THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded in 1887 by G. STANLEY HALL

Vol. XLV JULY, 1933 No. 3

WEIGHT-DISCRIMINATION WITH A VARYING STANDARD

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Experiments in discrimination or comparison are usually conducted by repeatedly presenting to a subject pairs of stimuli, one of which, called the standard, remains the same in all pairs. It is possible, therefore, that the subject's judgment concerning the members of any pair may depend to a considerable extent upon the accuracy of his attunement or adjustment to the constantly repeated standard. The subject may become 'set' so as to be prepared for the presentation of the familiar standard, as either the first or second member of the pair. There are indications, such as the phenomenon of absolute judgments in experiments entailing series of weight comparisons, that these 'sets' play an important rôle in the mechanism of comparison. To obtain facts by which to estimate the importance and significance of this factor of set in the process of comparison was the purpose of the present investigation. It seemed that a fairly direct way of studying the matter would be to compare the results of series of comparisons in which the standard was kept constant with the results of other series in which from pair to pair the standard varied irregularly over a wide range of intensities. Obviously the subject could not become adapted or 'set' with respect to the standard if the latter varied greatly from one comparison pair to another; so that such an experiment should at least bring about variation in set—and to bring about variation in any factor under study is the first prerequisite to scientific determination of its function. Such an experiment was accordingly carried out, with lifted weights used as stimuli. With both the constant and varying standard the method of constant stimuli was employed, and in general the series of the two sorts were obtained under conditions so nearly equivalent that any difference in outcome could be attributed to the matter of constancy or variation in the standard.

* Accepted for publication February 6, 1932.

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The data obtained with either the fixed or varying standard consist solely of the obtained percentages of reports of the three categories, heavier, lighter, or equal. These percentages yield two sorts of facts: one of these pertains to what may be termed the subjects' keenness or accuracy of discrimination; the other, and probably the more important, deals with the time-order error, that is, the difference between the percentages of correct reports concerning the same pair of stimuli depending upon the order in which the stimuli are given. It is believed that both sets of facts, particularly those concerning the time-order error, constitute important tests of the validity of any theory of the mechanism of successive comparison. The theoretical significance of these time-order errors has already been fully recognized by Köhler.[1]

Method and Procedure

As has been indicated, this study consists in a comparison of two procedures, the procedure with a fixed standard (the customary procedure) and that with a varying standard. A brief description will therefore be given of these two procedures and their differences.

With the first procedure two experiments were made, one with a standard of 110 grm. and the other with a standard of 200 grm. Each of 5 *Ss* was given 4 sittings of 100 trials each, in the case of both the standard of 110 grm. and that of 200 grm., a total of 800 trials with the fixed-standard procedure. Each was also given 800 trials, at 8 sittings of 100 trials each, with the varying standard. In the case of the varying standard, the standard varied irregularly from trial to trial by steps of 10 grm. from 110 to 200 grm. inclusive, and the trials at any one sitting included an equal number with each of the 10 standards (110, 120, 130, etc., to 200 grm.). Thus the varying standard took 10 different values, *i.e.* it consisted of 10 different standards, all used at any one sitting and mixed in an irregular order. Five variables were used in the case of both procedures, these variables differing from the standard with which they were compared by the following percentages: −3; 0; +3; +6; and +9. This asymmetrical and somewhat abbreviated series of variables sufficed for the present purposes and yet allowed a considerable saving of time as compared with a series containing as many variables lighter as heavier than the standard.

To equalize the effect of practice upon the two procedures, these were used in the order A A′ B B B B A′ A A A′ B B B B A′ A, in which A and A′ represent sittings with a fixed standard, A with the standard 110 grm. and A′ with the standard 200 grm., and B represents series with a standard varying from 110 to 200 grm. With both the fixed and varying standard, the order of standard and variable at any trial, from the first to the one hundredth or last, was the same at all sittings. The order in which the 5 variables succeeded each other was also the same for all sittings.

[1] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forschung*, 4, 1923, 115-175.

In order to avoid any misunderstanding of the two procedures, the actual order in which the weights were presented will be indicated by listing the weights used during the last 10 trials of each day's sitting.

In the above table the weight of the variable is indicated as the weight of the standard plus or minus a certain percentage. If the absolute weights of the variables were given, there would appear only 5 weights in the case of either of the columns for the fixed standard; namely, the weights 106.7, 110.0, 113.3, 116.6, and 119.9 grm. in the case of sittings 1, 8, 9, and 16, and the weights 194, 200, 206, 212, and 218 grm. in the case of sittings 2, 7, 10, and 15. In the case of the varying standard, however, there would appear 50 different weights, since there were 5 variables for each of the 10 standards. Thus the variable appearing in the above table as 120+9% had a weight of 130.8 grm.; that of 180+9% a weight of 196.2 grm.; etc. The important point is that in all sittings corresponding trials always involved a comparison of 2 weights of the same relative or percentage difference and with the standard and variable given in the same order.

Standard	Fixed				Varying	
Sittings.....	1, 8, 9, 16		2, 7, 10, 15		3, 4, 5, 6, 11, 12, 13, 14	
Order.....	1st	2nd	1st	2nd	1st	2nd
Trial 91.....	110	110+0%	200	200+0%	140	140+0%
Trial 92.....	110+3%	110	200+3%	200	170+3%	170
Trial 93.....	110+9%	110	200+9%	200	180+9%	180
Trial 94.....	110+3%	110	200+3%	200	200+3%	200
Trial 95.....	110	110+0%	200	200+0%	180	180+0%
Trial 96.....	110+9%	110	200+9%	200	120+9%	120
Trial 97.....	110	110+9%	200	200+9%	200	200+9%
Trial 98.....	110−3%	110	200−3%	200	150−3%	150
Trial 99.....	110	110+0%	200	200+0%	160	160+0%
Trial 100....	110+6%	110	200+6%	200	130+6%	130

The time elapsing from the moment when the *S*, after lifting the first weight of a pair, replaced it on the table to the moment he began lifting the second weight was approximately 5 sec.; and since the *S* used approximately 2 sec. to raise and 2 sec. to lower a weight, the time between corresponding phases in the two stimulation processes set up by any one pair of weights was approximately 9 sec. The time elapsing between the release of the second weight of one pair and the beginning of the lifting of the first weight of the next pair was approximately 10 sec.

The weights were made of shot and paper loaded in small, uniform bottles. In the case of the regular fixed-standard procedure, where the same standard was used repeatedly, 4 duplicate standards were employed in rotation so as to prevent identification of the standard by any possible change in its temperature or dryness as compared with the variables. The weights were lifted by being grasped by the thumb and the first two fingers in a manner carefully explained and taught to all the *S*s. The rate and height of lifting were not rigidly controlled by mechanical means, since such control is bound to introduce a distracting influence, but all the *S*s were given preliminary practice in lifting so that they took 2 sec. in raising the arm and 2 sec.

in lowering it. They also were instructed to aim at the greatest possible uniformity in the height of lifting, though this height was allowed to vary slightly in the case of the different *S*s. Any deviations from regularity in an *S*'s lifting movements noted by *E* were immediately corrected by calling *S*'s attention to them. *S* sat in a comfortable position alongside a table, his elbow resting on heavy felt. Screens erected at the edge of the table prevented *S* from seeing his hand, the weights, or any of the manipulations of *E*.

The *S*s were instructed to say "heavier," "lighter," or "equal," depending upon whether the second weight of a pair seemed heavier, lighter or equal when compared with the first. They were requested always to endeavor to make a judgment of heavier or lighter and to reserve the term equal for those cases in which they had a positive impression of equality, instead of employing it simply as a synonym for doubtful. In compiling the results half the number of equality judgments was added to the number of heavier and half to the number of lighter judgments. This procedure was followed merely in order to simplify the statement of the outcome of the experiments. The conclusions to be drawn would not be impaired had the equal judgments been considered separately or even entirely ignored. With the fixed-standard procedure there was a total of 492 or 12.3% of "equal" reports; with the varying-standard procedure, a total of 699 or 17.5%.

Results

So far as the main trends are concerned the data obtained may be shown by Tables I–III, in which the results for all 5 *S*s are pooled. No substantial exception to the general trends indicated by the pooled data is shown by the more detailed results by individuals given in Tables IV and V.

Table I is exceedingly simple and needs little explanation. It shows in what percentage of the trials the reports indicated that the variable seemed heavier, in each of two cases; namely, that in which the variable came first and that in which it came second. Since *S*'s report always referred to the second weight, there were no reports of "first heavier;" but it is possible to interpret the report "second lighter" to mean, or to be the equivalent of, "first heavier," and thus to determine in what percentage of trials the variable was indicated as the heavier when it was the first of the pair, and to compare this percentage with that obtained when it came second. It is necessary merely to remember that by the number of trials in which the variable was judged the heavier of the pair when it came first is meant the number of trials in which actually the standard, being second, was judged "lighter" (plus, as already stated, one half the number of "equal" judgments).

One other point in need of explanation is the manner of tabulating the results in the case of zero difference between the standard and variable. Since the distinction between variable and standard is entirely arbitrary in this case, there is no good reason for distinguishing between the case of variable first and

that of variable second. For the sake of uniformity in tabulation, the tables show, in the case of zero difference, under the heading "V 1st" in what percentage of the trials the first was indicated to be the heavier, and under "V 2d," in what percentage the second was reported heavier. To illustrate: in one-half the trials a bottle labelled V was given first, and in the other half of the trials such bottle was given second. The results were as follows, standard 110 grm.: bottle labelled V given 1st, first indicated as heavier in 41% of the trials and 2d indicated as heavier in 59%; bottle labelled V given 2d, first indicated as heavier in 39% of the trials and 2d indicated as heavier in 61%. It follows that the 2d was heavier on the average in $(59 + 61)/2$, or in 60%, and the first heavier in $(39 + 41)/2$ or in 40% of the trials. Thus the percentages given in Table I for zero difference and standard 110 grm. are, for V 1st, 40, and for V 2d, 60. This procedure has been followed in the case of all three tables. It results in the fact that the number of cases on which the percentages in each cell of a table are based is twice as great for zero difference as for the other differences, though the number of cases on which each row of a table is based is the same for all differences.

The form of tabulation adopted enables one to see at a glance, in the case of all variables, how much oftener the variable was indicated as heavier when it came first (column V 1st) than when it came second (column V 2d). The difference in the frequency with which the variable is reported heavier, according to whether it precedes or follows the standard, gives the time-order error. Thus, according to Table I, the variable weighing 3% less than the standard of 110 grm. was indicated as heavier than the standard in 28% of the 200 trials in which it came first and in 45% of the 200 trials in which it came second. In other words, the reports varied as though the same variable were heavier when second, or following the standard, than when first, or preceding the standard. Since, however, the variable was the same, the differences in its effect upon the reports must be due to the change in its temporal position. When the reports indicate that the variable seemed heavier when second than when first, *i.e.* when the percentage of "heavier" trials is greater in the case of the variable being presented second, the time-order error is labelled negative. In the reverse case, *i.e.* when the variable influences the reports as though it were heavier when given first, the time-order error is labelled positive. In the tables the difference in the percentages due to temporal position are given in the column headed "D," which stands merely for 'difference;' but this difference may be used as a quantitative description, though not as an absolute measure, of the direction and magnitude of the time-order error.

Table I, representing the outcome of the two experiments with the orthodox fixed-standard procedure, does not, before comparison with Tables II and III representing the outcome of the varying-standard procedure, bring out anything of particular interest, unless it be that it shows in somewhat emphatic form the existence of a pronounced negative time-order error. In the case of both standards, and every *S* and every variable (a total of 50 cases), the time-order error was negative. On the average, the

time-order errors with the two standards are very similar. The percentage of 'heavier' reports with the variable second exceeds that with the variable first, taking the average for all variables, by 18% with the standard 110 grm., and by 19% with the standard 200 grm. There is some tendency for the size of the time-order error to decrease with the increase in the size of the difference between standard and variable. Of course with very large differences the time-order error should disappear, since the judgments should be nearly always correct irrespective of the order of sequence of standard and variable. It is noteworthy, however, that the time-order error

TABLE I

DATA WITH CONSTANT STANDARD SHOWING THE EFFECT UPON THE PERCENTAGE OF REPORTS SIGNIFYING VARIABLE HEAVIER OF PRESENTING THE VARIABLE FIRST OR SECOND

The numbers in the columns "V 1st" and "V 2d" are the percentages of trials the variable (V) was indicated as the 'heavier' (including half the 'equal' judgments) in the case of variable 1st and variable 2d, respectively.
The number of trials on which each percentage is based is 200; the total number of trials represented by the entire table is 4,000.
The values in the column "d" show by what percentage the weight of the variable differed from that of the standard.
In the column headed "D" is given the difference in the percentages of heavier judgments due to the temporal position of the variable.

d	Standard, 110 grm.			Standard, 200 grm.			Both Standards
	V 1st	V 2d	D	V 1st	V 2d	D	Av. D
−3%	28	45	−17	28	45	−17	−17
0	40	60	−20	37	63	−26	−23
+3%	56	73	−17	56	77	−21	−19
+6%	65	86	−21	74	89	−15	−18
+9%	78	91	−13	81	97	−16	−15
Av.			−18			−19	−18

is still considerable with a difference (+9%, standard 200 grm.) which, when the variable comes second, gives as high as 97% correct judgments.

Table II gives the results obtained with the varying-standard procedure. No explanation of this table is needed since it is constructed in exactly the same way as Table I. The results with the varying standard agree with those obtained with a fixed standard in showing a pronounced negative time-order error with all differences. The average time-order effect is somewhat greater with the fixed standard than with the varying standard, the average difference in the heavier percentages due to time-order being 18 with the fixed standard, and 14 with the varying standard. The explanation of this difference between the two procedures in the time-order error with zero difference is not altogether clear. It may be in part due to 'ac-

cidental variation,' but it probably depends mainly upon the fact, which will be revealed by Table III, that in the case of the varying-standard procedure, the time-order error with any given variable is an average of time-order errors which differ enormously both in size and direction with the weight of the varying standard. It will be shown that with the varying standard the time-order error is largely a function of the weight of the standard.

TABLE II

DATA WITH VARYING STANDARD SHOWING THE EFFECT UPON THE PERCENTAGE OF REPORTS SIGNIFYING VARIABLE HEAVIER OF PRESENTING THE VARIABLE FIRST OR SECOND

Each cell shows the percentage of reports meaning variable heavier (including half the 'equal' reports)
No. of trials: each cell, 400; total 4,000.
Standard, varied from 110 to 200 grm.

d	V 1st	V 2d	D
−3%	36	48	−12
0	44	56	−12
+3%	53	69	−16
+6%	64	81	−17
+9%	76	91	−14
Av.			−14

In order to compare the keenness or the accuracy of discrimination in the case of the two procedures, it will suffice to give the percentage by which the variable had to exceed the standard in order to be indicated as heavier in 75% of the trials. The values have been calculated by linear interpolation between the two variables giving percentages of heavier lying immediately above and below 75. The results, with the differences between variable and standard expressed as percentages of the standard, are as follows.

Standard.....	Fixed at 110	Fixed at 200	Av. of Fixed	Varying
V 1st.....	8.31	6.43	7.37	8.75
V 2d.....	3.46	2.57	3.02	4.50
Av.....	5.89	4.50	5.20	6.63

Now with the varying-standard procedure the standards varied by steps of 10 grm. from 110–200, so that the average of all the varying standards exactly equals the average of 110 and 200. Except, then, for an effect due to difference in the two procedures, one might expect that the difference limen with the varying-standard procedure should not differ significantly

from the average difference limen with the fixed standards 110 and 200 grm. As a matter of fact, however, with a varying-standard the limen is considerably greater than the average limen for the two fixed standards, the former being 6.63% while the latter is only 5.20%. The former is 27.5% greater than the latter. It appears, then, that with a standard varying over a range of 110–200 grm. the difference limen is about 27.5% greater than it is on the average in this same region with a fixed or constantly repeated standard. Incidentally it may be observed that with the fixed standard of 110 grm. a larger relative difference limen was obtained than with the fixed standard of 200 grm.

The fact that the difference limen is decidedly greater with a constantly varying than with a fixed or constantly repeated standard means of course that the percentage of correct judgments with a given difference between two stimuli is by no means solely a function of the difference between the stimuli. Likewise it cannot be solely a function of the difference in intensity of two sensations corresponding to the two stimuli. Clearly the percentage of correct reports depends upon some consequence of keeping the standard constant or not doing so. The consequence of a constant standard may very well be some sort of adaptation or attunement to the standard; and in that case the percentage of correct reports would reflect to a large extent merely the accuracy or precision of attunement. By this is not meant that, with a given *S* and a given testing procedure, the correctness of judgment is not primarily a function of the percentage difference between variable and standard. The data behind Fechner's law is sufficient to indicate that, aside from well known deviations, this is approximately the case. But the percentage of correct reports obtained from different individuals or the same individual under varying circumstances is certainly not simply a function of the difference between the two stimuli or their corresponding sensations. It depends rather upon a complex internal operation which may be termed the operation of discrimination or comparison; and the preceding data may be interpreted as indicating that a very important determinant of the effectiveness of this operation is some factor which is seriously affected by the change from a constant to a varying standard. The point that it is desired to emphasize, then, is that the preceding data show definitely the influence upon the efficiency of the discriminal act of a factor independent of the difference between the two excitations.

The most important outcome of the present study does not consist in the results so far set forth, but in the facts concerning the relation of the temporal-order error to the weight of the standard in the case of the varying-standard procedure. With the varying-standard procedure, it will

be remembered, the varying standard really consisted of 10 different standards used in irregular order. In order to study the relation of the reports to the weight of the standards, the reports obtained with each of the 10 standards were tabulated separately, and then, to secure larger numbers and, consequently, greater reliability for the percentages of the various categories of reports, the data for each pair of adjacent standards were pooled. The data obtained with the varying-standard were thus analyzed or dismembered so that the results could be tabulated separately, first for the two lightest standards, 110 and 120 grm., then for the next heavier standards, 130 and 140 grm., and so on up to the two heaviest standards, 190 and 200 grm. For each of these 5 pairs of standards, the percentage of 'variable heavier' reports obtained with each variable was calculated separately for the two time-orders. The differences in the percentages of 'variable heavier' reports due to the time-order are shown in the lower part of Table III in the columns headed "D." Thus, in the case of the trials in which the standard was 110 or 120 grm. (trials scattered irregularly through the series of trials with varying standard), the variable which was 3% lighter than the standard was indicated as heavier in 55% of the trials in which it was presented before the standard and in 32% of those in which it was presented after the standard. The difference between these two percentages, which indicates the magnitude of the time-order error, is 23, a figure which is given in column D for the variable —3% and standards 110 and 120 grm. In studying the table, it is necessary to keep in mind that the plus and minus signs before the time-order errors in the columns headed D indicate whether the time-order errors are positive or negative. The error is arbitrarily labelled positive when the variable is indicated as the heavier more often when presented first, and negative when it is more often judged heavier when given second.

There are certain very decided and interesting phenomena shown by Table III. The facts may be stated in three general propositions.

(1) In the first place, it is clear that *with the varying standard the time-order error varies enormously with the weight of the standard.* It has been shown by Table II that when data for all the 10 standards constituting the 'varying standard' are massed together, the time-order errors come out negative for all of the variables. Table III, however, shows that the average negative time-order error indicated by Table II is a composite of very large negative time-order errors and smaller but definite positive errors. With the heavier of the 10 standards used in the varying procedure, the time-order is decidedly negative; while with the lightest standards, namely 110 and 120 grm., and with these only, it is positive. For example, the

data with zero difference in the case of the standards 110 and 120 grm. show a positive time-order error which corresponds to a change of 16 in the percentage of reports of 'variable heavier,' while in the case of the standards 190 and 200 grm., they show a negative time-order error corresponding to a change of no less than 50 in the percentage of such reports.

(2) The second noteworthy fact is that *the time-order error in the case of the 190 and 200 grm. weights with the varying-standard procedure is enormously greater than with the fixed standard.* With both procedures the

TABLE III

DATA WITH VARYING STANDARD SORTED TO SHOW THE DEPENDENCE OF THE DIRECTION AND AMOUNT OF THE TEMPORAL-ORDER ERROR UPON THE WEIGHT OF THE STANDARD

Each cell shows the percentage of reports meaning variable heavier (including half the 'equal' reports).

No. of trials: each cell, 80; entire table, 4,000

d	Standard in grm.									
	110 and 120		130 and 140		150 and 160		170 and 180		190 and 200	
	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d
−3%	55	32	33	35	39	40	25	54	20	77
0	58	42	51	49	46	54	44	56	25	75
+3%	73	54	61	54	45	74	61	81	34	83
+6%	83	61	64	76	64	88	61	83	49	96
+9%	86	85	88	83	83	93	60	93	58	98
	D		D		D		D		D	
−3%	+23		−2		−1		−29		−57	
0	+16		+2		−8		−12		−50	
+3%	+19		+7		−29		−20		−49	
+6%	+22		−12		−24		−22		−47	
+9%	+1		+5		−10		−33		−40	
Av.	+16		0.0		−14		−23		−49	

error is negative, but with the fixed-standard procedure, in the case of the standard 200 grm., this error corresponds to a change of 19 in the average percentage of heavier judgments with change in the temporal position of the variable whereas with the varying procedure the change in the average percentage reaches the almost unbelievable figure of 49. With standards 190 and 200 grm., varying-standard procedure, when there is no difference in weight between the standard and variable, the variable is reported heavier in 75% of the trials when it follows the standard and in only 25% when it precedes the standard. If a difference be termed liminal when it is noticed in 75% of the trials, it may then be said that with standards 190 and 200

TABLE IV

DATA WITH VARYING STANDARD SHOWING THE NUMBER OF "VARIABLE HEAVIER" JUDGMENTS FOR EVERY *S* AND EVERY DIFFERENCE FOR THE VARIOUS WEIGHTS OF THE STANDARD, FOR EACH TEMPORAL POSITION OF THE VARIABLE

No. of trials: each cell, 16; entire table, 4,000.

Standard	*d*	Subjects									
		Sy		*Mn*		*Ke*		*Ws*		*Fn*	
		V 1st	V 2d	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d
110 and 120	−3%	10	3	5	7	9.5	7.5	8.5	4	11	4
	0	11	8	8.5	8	12	4	10.5	7.5	9	11
	+3%	12.5	6	12.5	12	12.5	10	10	8.5	11	7
	+6%	11	6.5	13	12	14.5	9	14.5	9.5	13	12
	+9%	15	13.5	14.5	12	12	14	14.5	13.5	13	15
	Total	59.5	37	53.5	51	60.5	44.5	58	53	59	49
130 and 140	−3%	4.5	5	5.5	6	6.5	1	5	6	5	10
	0	8.5	7.5	5.5	10	11.5	5.5	9.5	7	3	7
	+3%	11.5	3.5	10	9.5	11.5	6.5	7.5	10.5	8	13
	+6%	12.5	14	9	13	12	12.5	10	9.5	8	12
	+9%	15.5	15	15	14.5	12.5	9.5	13.5	14.5	14	13
	Total	52.5	45	45	53	54	35	45.5	47.5	38	55
150 and 160	−3%	8.5	5.5	4.5	4.5	5.5	6	5	10	8	6
	0	6	5.5	7.5	8	8.5	8	6	9	6	10
	+3%	8	12.5	10.5	10.5	7.5	12.5	7	11	3	13
	+6%	8.5	13.5	11	14	11.5	16	10.5	14	10	13
	+9%	13.5	15	15	16	15.5	16	11.5	15	11	12
	Total	44.5	52	48.5	53	48.5	58.5	40	59	38	54
170 and 180	−3%	3	10.5	6.5	5	2	7.5	3.5	12.5	5	8
	0	5	5.5	7	12	3	4.5	9	9	12	14
	+3%	8.5	13	13	11	7.5	13	9.5	12.5	10	15
	+6%	8	14	11	13.5	11.5	12	11.5	15	7	12
	+9%	11.5	15.5	11	15	9.5	16	11	13	5	15
	Total	36	58.5	48.5	56.5	33.5	53	44.5	62	39	64
190 and 200	−3%	3	13	4.5	12.5	4.5	13.5	1	9.5	3	13
	0	2.5	13	1.5	11	5.5	11.5	2	10.5	5	11
	+3%	7	14	7.5	13.5	5.5	12.5	4.5	15	3	11
	+6%	6	15	11	15.5	9	15	8.5	15	5	16
	+9%	7.5	15.5	11.5	14.5	12	16	8	16	7	16
	Total	26	70.5	36	67	36.5	68.5	24	66	23	67

grm. a difference of zero constitutes the lower limen when the variable is first but the upper limen when the variable is second! The time-order thus has an effect equal to the difference between the lower and the upper difference limens. What would be the upper difference limen with the

variable first can only be estimated, since with the heaviest variable used, +9%, the variable was indicated as the heavier in only 58% of the cases. This same variable was judged heavier in 98% of the trials in which it came second. Obviously, the varying-standard procedure is a technique which enormously enhances the negative time-order error in the case of the heaviest of those standards which constitute the varying standard.

TABLE V

DATA WITH CONSTANT STANDARD SHOWING THE NUMBER OF "VARIABLE HEAVIER" JUDGMENTS FOR EVERY *S* AND EVERY DIFFERENCE

No. of trials: each cell, 40; entire table 4,000.

Standard	d	Subjects									
		Sy		*Mn*		*Ke*		*Ws*		*Fn*	
		V_1	V_2	V_1	V_2	V_1	V_2	V_1	V_2	V_1	V_2
110 grm.	−3%	13	16	12	15	8.5	17	13	19	9.5	22.5
	0	15.5	24.5	18	24	22.5	26	13.5	23	13	24
	+3%	21.5	27.5	25.5	27.5	21.5	25	22.5	32	21	34.5
	+6%	21.5	37.5	28.5	36	26.5	29	24	37	28.5	32.5
	+9%	26	38	37	38.5	32	29.5	27	40	33	36
	Total	97.5	143.5	121	141	111	126.5	100	151	105	149.5
	%	48.8	71.8	60.5	70.5	55.5	63.3	50	75.5	52.5	74.8
200 grm.	−3%	14	18	10	18	10.5	15	9.5	20	12.5	19.5
	0	17	26	11.5	26	16	23	17.5	25.5	13	25
	+3%	19	28	22	32.5	25.5	32	23.5	30.5	20.5	31
	+6%	23.5	36.5	32	33.5	32	33	30.5	37.5	29	36.5
	+9%	33	39.5	30.5	38	33	39	37.5	39.5	28.5	38.5
	Total	106.5	148	106	148	117	142	118.5	153	104.5	150.5
	%	53.3	74	53	74	58.5	71	59.3	76.5	52.3	75.3

(3) A third interesting characteristic of the time-order errors with varying standard is that *while they change from positive with the lightest standards to negative with the heavier ones by a fairly regular progression, they are not distributed symmetrically about the standard of middle weight.* The standard above which the errors are negative and below which they are positive is not the standard which lies at the middle of the series, but one which lies half way between the middle and the lightest. The standards lying in the middle of the series are 150 and 160, but these show a negative time-order error. It is only in the region of the standards 130 and 140 grm. that the time-order error is zero. Positive time-order errors were shown only by the one pair of standards lying below this region; namely, 110 and 120 grm., while negative time-order errors prevailed with the

three pairs of standards lying above it; namely, 150 and 160 grm., 170 and 180 grm., and 190 and 200 grm.

Such asymmetrical or unbalanced distribution of the time-order errors would be the consequence if the errors were the resultant of two causes; first, one producing a negative error throughout, independently of the weight of the standard; and secondly, one causing increasingly positive or negative errors with increase in the difference between the weight of a standard and the middle or average weight of all the standards. The errors due to the first cause would remain the same for all weights of the standard, whereas those due to the second would vary symmetrically about the middle weight. The resultant of two such causes would yield errors approximating those obtained. For example, let the first error be assumed to be constant for all standards and to have the magnitude of —14, which is the average negative error found with the varying-standard procedure taken as a whole. Let the second cause be assumed to produce no error at the middle of the series but one progressing along a straight line from +30 at the lighter end to —30 at the heavier end. The resultant errors would be distributed in a manner roughly approximating that of the obtained errors, and showing the same asymmetry. The following tabulation illustrates this fact.

Standards	Error A (constant)	Error B (symmetrical)	Resultant error	Obtained error
110 and 120	—14	+30	+16	+16
130 and 140	—14	+15	+1	0
150 and 160	—14	0	—14	—14
170 and 180	—14	—15	—29	—23
190 and 200	—14	—30	—44	—49

No theory of the process of comparison can be expected to explain all the details and irregularities of a mass of experimental data; but the asymmetry of the time-order errors mentioned above is such a striking phenomenon, and so reliably indicated by the general trend of the errors as shown by the whole table, that it seems that any adequate theory should be able to take account of it.

Theoretical Discussion

The facts concerning temporal-order errors now cover such a complicated and important set of phenomena that they must be regarded as constituting one of the chief tests of the validity of any theory of the comparison of successive intensities. Of the numerous theories concerning this operation

the one which attempts most definitely to explain these facts is that proposed by Köhler.[2]

While it may not be altogether clear how Köhler's theory would explain the particular facts set forth in the preceding part of the present paper, it seems probable that this theory, if aided by the proper subsidiary assumptions, would lead to expectations in fair harmony with the present data. Köhler's theory has not, however, met with universal indorsement. Fernberger, for example, whose investigations of weight discrimination have been extensive, writes as follows:[3] "This [Köhler's] theory presents certain highly acceptable features. There are other aspects of the present experiment, however, which suggest the possibility of another interpretation." "It is obvious that the *O* might develop a muscular set with which he approached each successive weight of the sort described by Martin and Müller." "It seems reasonable to proceed on the basis of the older theory before propounding a new one." The writer agrees with Fernberger that the older theories, making use of a concept of "set" should not be too hastily discarded. Yet if the older concept is to be regarded as affording a possible source of explanation of the facts of comparison, it must be developed in much greater detail than heretofore. In attempting to work out the theory of set so as to explain the time-order errors, however, the writer has found that it is necessary to make assumptions that from the purely logical viewpoint are very similar to those of Köhler's theory. The theory arrived at substitutes a concept of "set" for Köhler's "traces" and yet otherwise appears to leave intact the greater part of Köhler's theory. Whatever may be the value of such a compromise, since it is not clear just how any theory hitherto proposed would explain the facts concerning the time-order errors which have been described in the present paper, the theory to be sketched will at least have the merit of indicating one way of explaining the facts.

Probably general assent may be secured for Köhler's statement that a comparison of two excitations is itself one unitary event occurring at the instant of the second excitation. Moreover, the happening at the moment of the second excitation is not anything like a viewing by an internal eye alternately of the memory image left by the first excitation and of the actual perception aroused by the second excitation. Introspective reports have thoroughly established the fact that the presence of an image of the first impression is not essential to the comparison. In the case of comparison of two successive sound intensities, Köhler insists that there need be no consciousness of the first present at the instant that the second is experienced. Introspective reports in the case of weight discrimination have repeatedly established the same thing. The first must leave *some* effect, otherwise comparison would be impossible; but introspective evidence is conclusive that this effect is not of a sort which results necessarily in the first experience being continued either as perception or image. The fundamental question, then, concerns the nature of the after-effect left, or set up, by the first excitation.

According to Köhler's theory, the first excitation leaves a "spur," or trace.

[2] *Op. cit.*

[3] S. W. Fernberger, On absolute and relative judgments in lifted weight experiments, this JOURNAL, 43, 1931, 576.

This trace consists of an accumulation of positive H-ions set loose rapidly by the excitation, and has the remarkable property of "quietly representing" the excitation. It constitutes a "still after-effect" or "still image."[4] This after-effect is not the correlate of any sensory-experience such as sound or weight. It merely "represents" this experience. The accumulation of H-ions is assumed to reach its highest level about 1½ sec. after the cessation of the first stimulus (in the case of sound-intensity discrimination). At this highest level, the accumulation represents an intensity greater than that of the stimulus. Thereafter, naturally, there is a gradual dispersion or absorption of the H-ions so that the "level" of the accumulation gradually sinks. The rate of sinking is slow, and according to a hypothetical curve sketched by Köhler for illustrative purposes, strange to say, never would reach zero. If the accumulation of H-ions existing shortly after excitation is posited as representing the intensity of the excitation responsible for it, then obviously, as the level of the accumulation sinks, it may be said to represent a progressively decreasing intensity. With the lapse of time the trace left by a strong excitation becomes identical with that of a more recent, weaker excitation. The only factor coming into play to bring about comparison at the time of the second excitation is this trace at the level at which it exists at the instant of the second excitation. The impression made on *S* by the first excitation has nothing to do with the comparison except in so far as it affects the condition of the trace at the instant of the second excitation. It must follow, then, that a given second excitation will result in the same judgment when the first excitation is of high intensity but precedes the second by a considerable time as when the first is of moderate intensity but more immediately precedes the second. There is no memory or other awareness of the original condition of the trace; nor does the comparison process involve any reckoning by *S* of the influence upon the trace of the lapse of time since the stimulus responsible for it. *S*'s judgment depends upon the relation between just two things: the first, which may be termed L, is the intensity represented by the trace *at the moment of the second excitation*, and the second, which may be termed E_2, is the intensity of the second excitation. When L is less than E_2, the intensity of the second excitation is experienced as a "springing upwards," and presumably results in the report "second more intense." When L is greater than E_2, the judgment is "second less intense," and if L is equal to E_2, the judgment is "second equal." Since the level of the trace sinks during the interval between the first stimulus and the second, it follows that if the second excitation has the same intensity as the first, the second will meet with a trace-level, L, of lower intensity than its own intensity and therefore be experienced as a step upward and be followed by the erroneous judgment "second heavier." The theory thus explains the fact of the negative time-order error which is obtained under ordinary experimental conditions and also the increase in this error with increase in the lapse of time between the first and second stimulus. The general principle is that the less the concentration of +H-ions remaining as an after-effect of the first stimulus at the time of the second, the greater the relative over-estimation of the second, *i.e.* the greater the negative time-order error.

[4] For Köhler's use of these terms, "stillen Nachwirkung," and "stillen Bild," cf. *op. cit.*, 147 f.

Of course E_1, that is, the first excitation of a pair, would meet with the trace left by the preceding stimuli, and in particular by the trace left by the last preceding stimulus. Thus E_1 might meet with the same sunken trace level as E_2, and in such a case, if E_1 were of the same intensity as E_2, one might reason that since both E_1 and E_2 encountered the same trace level and were of the same intensity, they would be judged equal. Such reasoning would be incorrect, however, because it overlooks the fact that the judgment does not depend upon the relation of E_2 to E_1, but upon the relation of E_2 to the trace left by E_1 as it is at the moment E_2 occurs; and the condition of the trace at the moment of E_2 is by no means perfectly correlated with E_1, but depends upon many other factors, and especially upon the amount of time which has elapsed since E_1.

That Köhler's traces have the properties and results which he assumes cannot of course be directly proved or disproved. Some of the assumptions appear open to question. For example, it is rather difficult to understand how the amount of accumulated +H-ions left as a trace could ever come to represent the intensity of the excitation, since the trace as an index of intensity is utterly ambiguous. At a given instant the same trace might be the after-effect either of a very weak or of a very intense stimulus, depending upon how long previously the stimulus occurred. A long past, intense stimulus would have the same trace as a very weak but very recent one. No reckoning by *S* of the influence of the lapse of time upon the trace could very well be supposed to play a part in the process of comparison. Of course it may be pointed out that the time-order errors indicate that the trace represents the intensity of E_1 only very incorrectly. But while the time-order error is an important matter, the comparison is not always at the mercy of this error. The comparison may be correct in spite of the time-order error if the difference between the compared intensities is sufficiently great.

Whether or not Köhler's traces should lead to the expectation of errors in cases where as a matter of fact experiment does not reveal them is difficult, if not impossible, to determine, because the assumptions about these traces are not sufficiently definite. If, in the course of time, the trace of E_1 almost completely disappeared, then E_2, even though only a negligible fraction of the intensity of E_1 should by Köhler's theory be judged more intense, since the trace of E_1, having almost disappeared, would represent a lesser intensity than that of E_2. No one, however, has ever reported any conditions which will cause a weight of 10 grm. to be judged heavier than one of 1000 grm. If, however, one alleges, as Köhler apparently does, that the brain after experiencing an excitation never returns to its original condition as regards these accumulations of +H-ions, *i.e.* that the traces never vanish, or at least do not do so for weeks and months, then it would seem that in the course of one sitting the trace of any E_1 should become so inextricably mixed with that of innumerable other preceding E's that all comparison should be mere guess work. Traces varying simply as to different amounts of +H-ions accumulated in the same brain-region could not be kept distinct; the traces left by two excitations differing only in intensity would inevitably, as Köhler himself postulates, through some sort of addition,[5] become simply one trace.

[5] "Häufung von Einzelspuren," *op. cit.*, 173-174.

This assumption of the addition of traces, while necessary in order to explain the known effects upon any one comparison of the preceding comparison pairs, appears to lead to expectations contrary to all experience. One of a number of situations where the theory seems to lead to consequences contrary to fact, is that represented in Fig. 1.

The diagram represents the situation as it would be if an *S* were asked to compare E_2 with E_1, E_1 being preceded by the much more intense excitation E_x. The assumption is that *S* has been instructed to attend to all three weights, but to compare the third with the second. The dotted curve indicates the gradually sinking trace of the first excitation E_x, of an intensity of 200 grm. The intensity of the excitation represented by the trace as it exists 10 sec. after E_x is indicated by the diagram as having the hypothetical value of 193 grm. Now suppose that 10 sec. after E_x, E_1 occurred as a result of a stimulus of 50 grm. E_1 should apparently leave no trace of its own, but simply cause some increase in the residue of +H-ions left by E_x; so that 20 sec. after E_x the one existing trace should, because of E_1, represent a somewhat greater intensity than if E_1

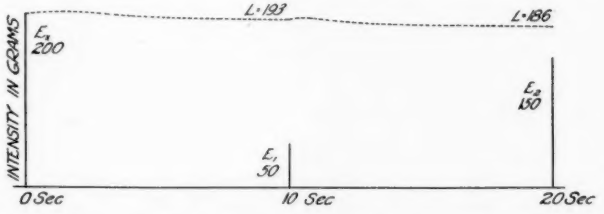

FIG. 1. DIAGRAMMATICAL REPRESENTATION OF SITUATION IN WHICH *S* COMPARES E_2 WITH E_1

had not been given. This intensity could hardly be much less than that produced by a weight 186 grm., as indicated in the figure, for the time error with a standard of 200 grm. and an interval of 20 sec. is probably not greater than 14 grm., *i.e.* the difference between 200 and 186 grm. It follows that if E_2 were an excitation due to a weight of 150 grm., its intensity would be less than that represented by the trace at the time of E_2, and the transition experience, being downwards, would result in a report that the third weight was lighter than the second. Now it is true, as the data given in this paper have shown, that in the comparison of relatively light weights preceded by relatively heavy weights, the second weight of the pair of light weights will be underestimated, *i.e.* the time-order will be positive. But it is inconceivable that in the case illustrated above this time-order error would become so great that the stimulus of 150 grm., producing E_2, would be judged lighter than the stimulus of 50 grm., producing E_1. Yet that is what should happen, so far as the writer can determine, according to Köhler's theory. The outcome of the judgment depends according to this theory upon the relation of the level of the trace at the moment E_2 occurs and the intensity of E_2. Neither memory-images of the preceding weights nor any allowance for lapse of time play a part in the comparison. Thus there seems no escape

from the conclusion that according to Köhler's theory 150 grm. can be made to be judged lighter than 50 grm. by having the latter weight preceded by a weight of 200 grm. Moreover, this outcome remains the same even if a weight of 1 grm. is substituted for that of 50 grm.

Whether or not the difficulties with Köhler's theory which have been mentioned could be cleared up by means of subsidiary hypotheses or by some re-interpretation of Köhler's statements, it remains true that the fundamental assumptions of Köhler's theory are far from proved and that it is therefore permissible to cast about for other explanatory concepts. As already stated it seems to the writer that the concept of set may be made to afford the basis of an explanation of comparison in a manner more satisfactory than Köhler's residues of +H-ions. According to this concept, the effect of the first of a pair of stimuli upon an *S* who has been informed that a second, or following, stimulus is to be compared with the first, is to set up in him a preparation for, adjustment or attunement to, or expectation of, a certain intensity of following stimulus. That such adjustments or expectations may exist without conscious imaging of the intensity adjusted to, is a familiar fact. For example, it is quite possible to arrange a discrimination reaction-time experiment in which *S* as the result of a few practice trials will react to only one intensity or to one pitch (within narrow limits). *S*, at the warning signal, must adopt some sort of expectancy of adjustment toward a stimulus of a particular character, and not simply a general readiness to react; for otherwise he would react to any stimulus instead of exclusively one of the prescribed character. Now in such cases introspective reports concerning the absence of an image of the prescribed stimulus may be just as emphatic as reports of the absence of an image of the first of a pair of comparison weights at the moment of presentation of the second.

Which of many terms is the best to use to describe the phenomenon, set, attitude, expectancy, adjustment, adaptation, or attunement is a matter of opinion. They are all admittedly vague, and any physiological account would be extremely speculative; but whatever word is chosen, its meaning, as here intended, is a readiness for a stimulus of a particular character. Introspective reports will vary concerning the presence or absence of imagery as present in this condition. Frequently, at least, they will indicate the existence of certain motor tensions. The writer believes that such a set, involving certainly non-projection areas and often, at least, motor areas, is a necessary postulate in order to understand comparison. Further, a few very simple assumptions concerning the behavior of properties of such sets

will account for all the known facts concerning the time-order errors in the comparison of successive intensities.

The attributes which must be postulated of this expectancy or set in order to explain the complicated facts of comparison, including those concerning the time-order error with both constant and varying standard, are as follows.

(1) In the case of the comparison of intensities, the set existing at the moment of the second excitation is a set towards a certain intensity. This intensity will be represented by the symbol L, and termed the level.

(2) The level of the set tends to approximate the average intensity of all the preceding stimuli of the entire series given from the beginning of the experimental sitting, providing the series does not vary over too great a range. The average of the entire series should be thought of as a weighted average, in determining which, the most recent stimuli have the greater weight and the last stimulus the greatest weight.

It follows, therefore, that the level adopted following a stimulus more intense than the average will at first be raised but will revert towards the average level. Conversely, the level, while suddenly lowered by an exceptionally weak stimulus, will tend to revert to the average. This assumption, which amounts to bringing in the hypothesis of assimilation, would explain the time-order errors in the case of the varying standard—negative when the varying standard is heavier than the average and positive when exceptionally light. It does not explain why the negative errors for the heaviest varying standard are so much greater than are the positive ones for the lightest varying standard. This fact, as well as the negative errors with the regular constant-standard procedure, require one more assumption.

(3) The level of intensity towards which *S* is set tends in all cases to sink towards a lower level, but does not even remotely approach a level of zero as long as the set is maintained. When *S* ceases to expect a second stimulus, then the set or preparation would presumably either disintegrate or fall back to some habitual level.

The plausibility of the above assumptions is a matter of opinion. Particularly might there be question concerning the third assumption, that the intensity towards which *S* is adjusted tends to decrease with the lapse of time before the expected stimulus. It is barely possible that this may be merely a special case of the second assumption, in that there may be a chronic level of adjustment corresponding to the average intensity of habitual stimuli; in which case the tendency of the level to sink would be merely the tendency to adjust to an average habitual intensity. In the case

of weight, this habitual level might be that corresponding to the weight of the fore-arm. Another possibility is that sets are constituted of motor strains and that a set to a high intensity means a high degree of strain. It is fairly plausible that a condition of high degree of tension, cerebral or muscular, should with the lapse of time tend to change into one of lesser tension. All that is here alleged, however, is that the known facts concerning comparison of successive intensities point to some attunement or set as an important factor in that act and that these facts may be accounted for by making the above assumptions concerning the behavior of the set.

The above schema, and it is only that, of a theory of comparison of intensities may be summarized very briefly. Calling the intensity of the first and second excitations E_1 and E_2, and the intensity level of the set, L, the judgment of the subject depends upon the relation of E_2 to the L existing at the instant of E_2. If E_2 is greater than L, it is experienced as an excitation of greater intensity, as a "rising up from," to use Köhler's expression. If E_2 is less than L, the second excitation is judged as less, and if E_2 is equal to L, as equal. The intensity level, L, *immediately after* E_1 is regarded as being approximately equal to the intensity of E_1. It may at first, within the first few seconds, even exceed that of E_1, thus constituting an over-adaptation or over-adjustment and leading to positive time-order errors. Normally, however, L decreases with the lapse of time—to what extent can be determined by working out the increase in the normal negative time-order error with increase in the interval between the two comparison stimuli.[6] Lastly, if E_1 is much higher than the average intensity of the series, the level L tends to revert rapidly from E_1 to the average; likewise, if E_1 is much less than the average, L tends to revert rapidly to the average, the reverting in this case however constituting a rise in L instead of the normal decrease.

The following diagrams, in which the height of the vertical lines represents the intensity of the excitation and the height of the dotted line the intensity to which the subject is set, will illustrate the chief phenomena of time-order errors.

Fig. 2 illustrates the explanation of the normal negative time-order error. E_2, though equal to E_1, is greater than L; therefore the judgment is "second heavier." Since the level L decreases with the increase in the length

[6] For data on this increase see: J. Borak, Über die Empfindlichkeit für Gewichtsunterschiede bei abnehmender Reizstärke, *Psychol. Forsch.*, 1, 1922, 374 ff.; K. Fodor, and L. Happisch, Über die Verschiedenheit der Unterschiedsschwellen für den Gesmacksinn bei Reizzunahme und Reizabnahme, *Arch. f. d. ges. Physiol.*, 197, 1922, 337 ff.; and Köhler, *op. cit.*

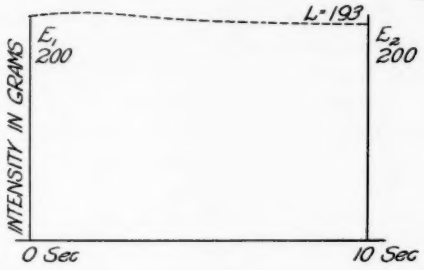

FIG. 2. DIAGRAMMATICAL ILLUSTRATION OF THE NORMAL NEGATIVE TIME-ORDER ERROR

of time between E_1 and E_2, the figure also illustrates the increase in negative error with increase in the interval between the two compared stimuli.

Figs. 3, 4, and 5 illustrate the chief facts in the case of a varying standard. In these three figures it is assumed that *S* has adapted himself to the

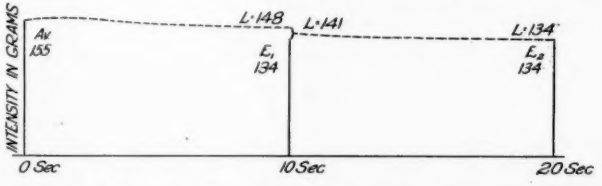

FIG. 3. DIAGRAMMATICAL ILLUSTRATION OF THE TIME-ORDER ERROR WHEN THE VARYING STANDARD IS SLIGHTLY LIGHTER THAN THE SERIES AVERAGE

average of the entire series, taken as 155 grm., and further, for the sake of illustration, that L drops the equivalent of 7 grm. during the 10 sec. between E_2 of one pair and E_1 of the following, and likewise during the 10 sec. between E_1 and E_2 of the same pair. It is further assumed that the

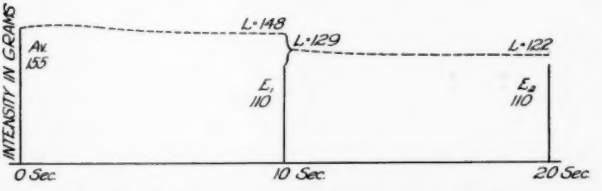

FIG. 4. DIAGRAMMATICAL ILLUSTRATION OF THE TIME-ORDER ERROR WHEN THE VARYING STANDARD IS MUCH LIGHTER THAN THE SERIES AVERAGE

height of L shortly after any E_1 may be taken as half way between the height of L at the moment E_1 occurred and the intensity of E_1, that is, that the last E and the whole preceding series have equal effect upon L, so that L is the average of what it would be if solely determined by the preceding series and what it would be if determined by the last E. This fusion level is likewise assumed to drop 7 grm. between E_1 and E_2. These assumptions, made for the sake of illustration, are undoubtedly inaccurate, and too simple, but they will serve to indicate at least the general direction of certain explanations. According to these assumptions there will result a set or a readiness for an intensity indicated at L by the height of the dotted line above the base-line. The judgment will depend upon the relative height of the level L and of the last excitation E_2. The diagrams thus illustrate the explanation of the following facts

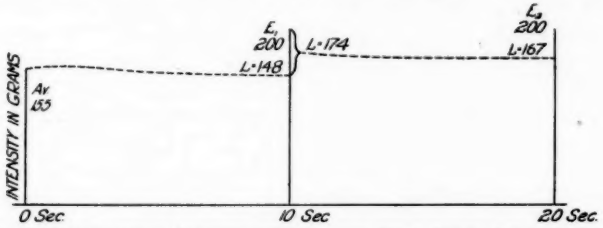

FIG. 5. DIAGRAMMATICAL ILLUSTRATION OF THE TIME-ORDER ERROR WHEN THE VARYING STANDARD IS MUCH HEAVIER THAN THE SERIES AVERAGE

concerning time-order errors with the varying-standard procedure: Fig. 3, the varying-standard when it weighs 134 grm. (lighter than the series average of 155 grm.) gives no time-order error, since L and the intensity of E_2 are the same; Fig. 4, the varying standard when very light (110 grm.) gives a considerable positive time error, since L is greater than E_2; Fig. 5, the varying standard when very heavy (200 grm.) gives a negative time-order error far greater than with the fixed-standard procedure (cf. Fig. 2), and also a negative error of much greater magnitude than that of the positive error shown by the 110 grm. standard (cf. Fig. 4).

The theory suggested, then, affords an explanation of the striking asymmetry of the time-order errors, in the case of the varying standard, when these errors are plotted in relation to the weight of the standard. Instead of being evenly balanced about the average weight of the whole series, they are distributed about an indifference weight considerably below the average. The explanation of this asymmetry, as likewise the fact that an exceptionally heavy pair of comparison weights shows a much larger

negative error than the positive error shown by an equally exceptionally light pair, lies in the assumption that the level, L, even when it corresponds to the average of the entire series from the beginning of the sitting, always sinks between E_1 and E_2. As already pointed out,[7] the asymmetrical distribution of the errors may be regarded as due to two factors, the first causing a uniform, negative, time-order error and the second an error symmetrically distributed, as regards its magnitude, about the middle weight of the series, though positive for standards below this weight and negative for standards above it. According to the above hypotheses the first of these factors would be the sinking of the level, L, which always occurs between E_1 and E_2; the second would be the tendency of an L which was deflected from the general level by an exceptionally light or exceptionally heavy E_1 to revert to the average level.

How, it may be asked, would the assumptions here suggested solve the dilemma illustrated in Fig. 1, in which, according to Köhler's theory, a weight of 50 grm. should be judged heavier than one of 150 grm.? As a matter of fact the assumptions used in the construction of Figs. 2–5 would result in the case of Fig. 1 in a height of L, at the moment of E_2, of only 114.5 instead of 186, so that a weight of 150 grm. would by these assumptions be judged not lighter, but much heavier than one of 50 grm. According to the assumptions, however, a weight of 50 grm. would be judged as heavy as one of 114.5 grm., and a weight of 1 grm. would be judged as heavy as one of 90 grm., both of which consequences, it would seem, could hardly agree with the experimental fact. It should be remembered, however, that attitudinal sets are more under the voluntary control of *S* than are accumulations of +H-ions due to physico-chemical laws. *S* cannot suddenly shake away the accumulation of H-ions left by a preceding relatively heavy weight. He would, however, experience the very weak excitation E_1 following the relatively very intense E_x, as an extremely light weight and might very well, as a consequence, adopt an attitude which did not assimilate with that produced by E_x, but was entirely separate from it, or even contrasted with it. It seems inevitable that when the range of stimuli is very great, a stimulus near either extreme of this range would result in an exception to the rule that *S* tends to adapt to the average of the entire series. It is for this reason that the statement in assumption (2) that the level of intensity to which *S* adjusts himself as the result of E_1 is the average level of the entire series, has been limited to series that do not vary over

[7] *Supra* 403.

too great a range. The experimental data given in the first part of this paper show that the range may be as great as that from 110 to 200 grm. Whether the results would remain the same, however, with the varying-standard procedure, in case the standard varied, for example, from 20 to 600 grm., only experiment can tell; but on general considerations one should expect radically different results.

There are many other facts than those concerning time-order errors which may be readily explained on the basis of the above assumptions, though undoubtedly a considerable number of factors not envisaged by these assumptions come into play in special instances. Among these subsidiary factors are the influence of preceding judgments (not merely preceding stimuli), changes in set due to variation in instruction, imagery, the effect of change in *S*'s understanding of the categories of judgment, etc. So far as the writer is aware there are no facts of comparison which can be accounted for by Köhler's concept of sediment-traces which cannot also be explained in terms of set; and many phenomena seem more easily understood by reference to the latter concept. For example, the size-weight illusion is ordinarily explained through the use of a concept of expectant attitude or set. The set is produced in this case not so much by the first stimulus-object of a pair as by the relative size of the second. If the second stimulus-object is larger than the first it arouses an expectation of a heavier weight, and if smaller, *S* becomes set for a lighter weight. In either case there is a discrepancy between the level, L, of the set and the intensity of the second weight. In the case of a large weight, the level L corresponds to an intensity greater than E_2, and therefore the weight of the large object is underestimated. Again, acknowledgment of the importance of sets or attitudes in successive comparison has the advantage of bringing the explanation of time-order errors in the case of intensity into line with those in the case of the comparison of short intervals of time. The effect of attitude on comparisons of interval has been experimentally investigated by the writer and found to be enormous.[8] The absolute indifference interval of about 0.6 sec. suggests a permanent natural set analagous to the more temporary attunement which it has been assumed above may be established experimentally by continued stimulation by a series of stimuli of not too wide a range of variation. Lastly, it may be suggested that explanations in terms of set or attitude probably account better for individual differences, contradictory judgments by the same

[8] H. Woodrow, The reproduction of short temporal intervals, *Exper. Psychol.*, 13, 1930, 496; Individual differences in the reproduction of short intervals, this JOURNAL, 45, 1933, 276-281.

individual, and significant variation from day to day,[9] than do traces consisting of accumulations of H-ions. Sets or attitudes are notoriously variable and unstable, whereas the immediate physiological effects of stimulation, while also variable, should on the whole lead to consequences of rather greater uniformity than the highly variable and contradictory judgments resulting from repeated comparisons. The lesser acuity in the case of a varying standard than in that of a fixed standard could be explained on the assumption that a set to an average of widely differing weights would be more indefinite and variable than in the case of a constantly repeated standard.

Summary

A comparison was made as regards keenness and time-order errors between two procedures for the study of discrimination of lifted weights. The two procedures, both following the method of constant stimuli, were alike in all respects except that in the first the standard was fixed or constant throughout a sitting while in the second it varied irregularly from trial to trial from one to another of 10 different weights. With the fixed-standard procedure experiments were made with standards of 110 grm. and 200 grm. With the varying-standard procedure the standard varied by steps of 10 grm. from 110 to 200 grm. The chief conclusions drawn from the experimental data, based on a total of 8,000 comparisons by 5 *S*s, were as follows.

(1) With the fixed standard procedure, all variables showed a *negative* time-order error. With the 110 grm. standard, variables were on the average judged heavier 18% more often when they followed the standard than when they preceded it, and with a standard of 200 grm., 19% more often.

(2) With the varying standard procedure, the time-order errors, while on the whole negative, varied enormously with the 10 different weights given the standard. At its lightest weights, 110 grm. to 120 grm., all variables yielded *positive* time-order errors (corresponding to a difference in the percentages of "variable heavier" of 13); at weights 130 to 140 grm., the variables yielded an average time-order error of zero; while at weights from 150 to 200 grm., inclusive, all variables yielded *negative* time-order errors. These negative errors showed a regular increase with increase in the heaviness of the standard, until with the varying standard at weights of 190 and 200 grm., the negative time-order error reached a remarkably great magnitude and far exceeded that with the fixed-standard

[9] Cf. Woodrow, Quotidian variation, *Psychol. Rev.*, 39, 1932, 254-256.

procedure. With the standard at these last mentioned weights, that is, 190 and 200 grm., a change in the order of the variable and standard caused a change of 49, on the average, in the percentage of trials in which the variable was judged heavier. For example, when the variable was 3% heavier than the standard, the variable was juged heavier in 83% of the trials in which it followed the standard but in only 34% of those in which it preceded the standard.

(3) The difference threshold, taken as a percentage of the standard, was about 27.5% greater for the varying standard than for the average of the two fixed standards.

(4) It is contended that the above facts have considerable significance for the theory of the comparison of intensities of successive stimuli, and that they may best be explained by assuming that the first stimulus of each pair is followed by an attitude of expectancy, or set, as regards the second. Three simple assumptions concerning the behavior of these 'sets' are shown to suffice for the explanation of the facts.

SERIAL LEARNING AND GENERALIZING ABSTRACTION

By LEON ARONS, University of Pennsylvania

While there have been a number of investigations of the higher mental processes,[1] little has been done specifically comparing (in quantitative terms) serial learning of the language type with generalizing abstraction. "The establishment of mere *chains* of verbal reflexes as in the learning of a string of nonsense syllables is one type of linguistic organization."[2] On the other hand, generalizing abstraction as a function of "symbolic intelligence," might be expected, to an extent, to be dependent upon degree of facility at acquiring these "mere chains of verbal reflexes." Briefly, our problem is to investigate the degree of correlation between an *S*'s ability at serial learning with his ability at generalizing abstraction as revealed by the "multiple choice" method.

METHOD AND PROCEDURE

Most investigations of learning (verbal) have made use of nonsense material. The comparative degree of nonsense of this material may be questioned.[3] In addition, for our purpose, it is most desirable to use a method which will give a wide range and distribution of scores. We have therefore chosen material (playing cards) which is relatively meaningless, and have placed the emphasis on acquisition in pattern (serial) form. Differences in 'card background' are minimally significant. All *S*s may be expected to know the names of the cards. This method will also give us our desired scatter of scores.[4]

There is a paucity of experimental data on generalizing abstraction. Moreover, the term itself is somewhat vague, or at least, too general in its connotation. The latter is especially true if we are to consider generalizing abstraction to be synonymous with concept, as Hull seems to do,[5] although at times he uses the term "general-

* Accepted for publication January 11, 1932. From the Psychological Laboratory of the University of Pennsylvania.

I wish to express my indebtedness to the many *S*s who served in this investigation, and to Professor Samuel W. Fernberger and Dr. Francis W. Irwin who offered invaluable criticism and advice.

[1] J. C. Peterson, Higher mental processes in learning, *Psychol. Monog.*, 28, 1920 (no. 129), 1-127; B. F. Haught, Interrelation of some higher learning processes, *ibid.*, 30, 1922 (no. 139), 1-71. For other references see these two studies.

[2] F. Lorimer, *The Growth of Reason*, 1929, 93.

[3] F. C. Davis, The relative reliability of words and nonsense syllables as learning material, *J. Exper. Psychol.*, 13, 1930, 221-234; F. M. Sauer, The relative variability of nonsense syllables and words, *ibid.*, 13, 1930, 235-246.

[4] Our method is similar to Renshaw's (cf. S. Renshaw, Experiment of the learning of 'paried associates,' *J. Appl. Psychol.*, 11, 1927, 226-233).

[5] C. L. Hull, Quantitative aspects of the evolution of concepts, *Psychol. Monog.*, 28, 1920, (no. 123), 1-85. For criticisms of methods used by Grünbaum, Moore, and Fisher, see Hull; for more recent references, see N. H. F. Maier, Reasoning in humans, *J. Comp. Psychol.*, 10, 1930, 115-144.

izing abstraction" interchangeably with "methods by which concepts are evolved." He lists three types: (1) a type exemplified by a child withdrawing from an electric glowing bulb after having been burned by a candle; (2) a type of "generalizing abstraction is involved in the so-called inductive 'steps' of formal instruction originated by Herbart," which he thinks "has strong elements of artificiality," and (3) the "standard" type, which he believes to be the method by which the great majority of actual concepts are evolved. In this latter type, the "formation of the concepts has never been an end deliberately sought for itself . . . always a means to an end—the supremely absorbing task of physical and social reaction and adjustment."[6]

We chose, secondly, the Yerkes multiple choice method,[7] which presents a number of novel settings (situations). One 'key' in each set will sound a buzzer. *S* collects his data (observations) so as to be able to formulate a rule (generalization) which will enable him to predict the buzzer key for any other setting given him. In other words, when the *S*'s behavior becomes 'adequately' generalized with respect to these situations (with the concomitant language 'key'), he is considered to have solved the problem.

Technique. (A) Serial learning. S, seated at a small table in a quiet room, with *E* opposite, was given a deck of playing cards (previously arranged) face down, and the following instructions.

Here is an ordinary deck of playing cards. You are to learn as many as possible in correct order. Here is how you are to go about it. When you are ready to start, hold the deck face up in the palm of your hand. Call out the top card, the name and the suite, then put it face down on the table in front of you. Then call out the card now on top, and put it face down on the first card. In this manner go through the entire deck. When you have finished going through the deck, I shall ask you to call off as many cards in correct order (starting from the first), as you can. Having recited, you will go through the deck again, and so on. After each leafing of the deck you recite. You have 15 min. to spend in actually going through the deck. Within the limits given by these directions, you can go about it in any manner you choose, as rapidly or slowly as you like, and so on. Don't cram, but go about it as efficiently as possible. Try to work out an optimum method for yourself of going about it. You are not to look back once you have put a card down, and you are to uncover but one card at a time. These cards are not arranged according to any key, so do not waste any time looking for one. Remember as many cards as possible, in correct order starting from the first.

At the end of the first "card period," the *S*'s method of going about the learning was discussed with him, and he was allowed to formulate what he considered the best way of going about it the next time. He was told that a different order of cards would be used the second time, and was asked not to practice in the interim.

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[6] It might be well for us to indicate what we mean by "generalizing abstraction." We would inscribe the highly popular adjective 'dynamic' into the matter, and consider generalizing abstraction as generalized behavior with a language (in the broadest sense) 'key' or 'tab.' In no sense do we intend to suggest a dichotomy between the 'behavior' and the 'key,' nor to reify either term. Behavior is 'generalized' with respect to certain situations. An aspect of the generalizing is the language 'key.' The key may be considered a formula of the whole of this behavior in symbolic terms.

[7] R. M. Yerkes, A new method for studying the ideational behavior of mentally defective and deranged as compared with normal individuals, *J. Comp. Psychol.*, 1, 1921, 369-394.

Time for each leafing of the deck was recorded (stop watch) along with the *S*'s recitations. When the remainder of the time prescribed permitted of but one leafing more, *S* was notified of the fact. The 15-min. periods were kept within 30-sec. limits. Two card periods were used, separated by approximately 48 hr. At the second period *S* was told to go about it on the basis of experience acquired the first time, otherwise the directions were the same. *S* was asked to describe his method of learning after each period.

The order of cards for each period was as follows.

Period 1. 3H, 7D, AH, 8C, KS, 10H, JC, 3S, 5D, 2C, 8D, 10S, 4H, KC, 7C, 4D, 9H, QC, 6D, AS, JD, 4S, 8H, QS, 6C, JS, KH, 3D, 5S, AC, 9S, JH, 4C, 2D, 9C, 7H, 10D, 5C, 2H, 8S, KD, 7C, 9D, 2S, QD, 5H, 10C, 6H, AD, QH, 3C, 6S.

Period 2. 9C, 5D, JS, 3C, AD, 5H, QS, 9D, 8C, 6D, 9H, AC, 6C, KH, 10S, 3D, 7H, 5C, JD, 2C, 8D, 4S, KD, AH, 8H, QH, 6H, QC, 2D, 4C, KS, 8S, 7C, 3S, 6S, KC, AS, 4D, 10H, 2S, 9S, 2H, 5S, 10C, 4H, JH, 7S, 10D, 3H, QD, JC, 7D.

(B) Multiple choice. The Yerkes multiple choice apparatus was used in this part of the experiment. The problems may be said to differ in two ways: (1) according to principle involved; and (2) according to point of reference or application. Such principles as middleness, alternation, number of groups, odd-and-evenness, etc. may be used, and such points of reference, which are more limited, as the ends, the center, etc. In our choice of problems we endeavored to make use of a different principle in every problem, and to avoid the mere complication of principles already given the *S*. Points of reference also were kept simple. We may, therefore, expect results which will give a fairly accurate 'picture' of the *S;* chance factors in the solution having been reduced to a minimum.

The principles made use of in these problems are not new to *S*. In his background will be found such abstractions as middleness, spacial progression, and the like. He must, however, arrive at a generalization adequate to the problem in hand. Here the matter of 'direction' of activities, relational discriminations, and so on, are involved.[8] The verbal generalization finally evolved is a product of these situations, and is an aspect of *S*'s 'generalizing behavior.' A list of the 6 problems used is given in Table I.[9] The explanatory portion following the problem number gives a statement of the solution. The settings are given in their order of presentation to the *S;* "1–7" indicates that keys one to seven inclusive were extended in this particular setting. The parenthetical number designates the correct key for this setting. Two series of numbers for correct keys will be found for Problem V. This problem is one of alternation, and inasmuch as there is an odd number of sets, repetition of sets after the 15 listed sets have been used, will bring about a shift in the keys correct for the next 15 sets. The correct keys for these sets (sets 16 to 30) will be found in the second column of parenthetical numbers. It should be stated that the series of sets listed were repeated. The maximum number of sets allowed will be discussed in a later section. The order of presentation of the problems is based upon a preliminary investigation on 20 *S*s. In this present investigation, the order of presentation was Problems I, III, II, IV, V, and VI.

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[8] Cf. Maier, *op. cit.*

[9] Four of our problems were used by Yerkes: Problem 1—Yerkes No. 3; Problem II—Yerkes No. 6; Problem III—Yerkes No. 1; and Problem V—Yerkes No. 10.

TABLE I

THE MULTIPLE CHOICE PROBLEMS AND THEIR SOLUTIONS
(Figures in parentheses give correct key)

Problem I. Principle involved: middle key.

1.	1–7	(4)	3. 6–10	(8)	6. 4–10	(7)	9. 1–5	(3)
2.	10–12	(11)	4. 1–11	(6)	7. 1–9	(5)	10. 6–12	(9)
			5. 1–3	(2)	8. 9–11	(10)		

Problem II. Principle involved: starting with key 12, the next key to left at every trial.

1.	10–12	(12)	4. 8–12	(9)	7. 5–8	(6)	10. 3–7	(3)
2.	6–12	(11)	5. 8–10	(8)	8. 4–9	(5)	11. 1–5	(2)
3.	3–10	(10)	6. 1–9	(7)	9. 2–11	(4)	12. 1–12	(1)

Problem III. Principle involved: same key at every trial, i.e. no. 5 from left.

1.	1–6	(5)	3. 4–6	(5)	6. 4–5	(5)	9. 1–8	(5)
2.	3–12	(5)	4. 5–9	(5)	7. 4–10	(5)	10. 5–10	(5)
			5. 2–10	(5)	8. 3–6	(5)		

Problem IV. Principle involved: first key from right with even number of keys, and second key from right with odd number of keys.

1.	1–5	(4)	5. 4–7; 9–12	(12)	9. 3–12	(12)	13. 7–9	(8)
2.	3–8	(8)	6. 1–11	(10)	10. 1–3; 7; 9–11	(10)	14. 2–11	(11)
3.	5–6	(6)	7. 4–6	(5)	11. 2–9	(9)	15. 6–12	(11)
4.	8–12	(11)	8. 8–11	(11)	12. 6–7; 9–11	(10)		

Problem V. Principle involved: alternately the key to the left and right of the middle key.

1.	1–7	(3)	(5)	6.	3–9	(7)	(5)	11.	4–12	(7)	(9)
2.	8–12	(11)	(9)	7.	2–4	(2)	(4)	12.	3–5	(5)	(3)
3.	2–10	(5)	(7)	8.	6–10	(9)	(7)	13.	4–10	(6)	(8)
4.	10–12	(12)	(10)	9.	6–12	(8)	(10)	14.	5–9	(8)	(6)
5.	1–9	(4)	(6)	10.	3–7	(6)	(4)	15.	2–12	(6)	(8)

Problem VI. Principle involved: as many keys from the left as there are groups extended.

1.	1–6; 10–12	(2)	6.	3–10	(3)	11.	5–9; 12	(6)
2.	1; 7; 9; 12	(12)	7.	1; 5; 8	(8)	12.	2; 4–6; 8; 10; 12	(8)
3.	4–12	(4)	8.	1–3; 6–8; 10; 12	(6)	13.	1–5; 7; 11–12	(3)
4.	4–8; 11–12	(5)	9.	1; 3; 5; 7; 9; 11	(11)	14.	1; 4; 7; 9–12	(9)
5.	2–5; 8; 10–11	(4)	10.	1–12	(1)	15.	2; 4; 6; 8; 10; 12	(12)

The apparatus was placed on a small table. Its operation was explained to *S*, in the following manner.

See this setting? (*E* pushes out Set 1, Problem I) and this set? (*E* presents Set 2), and this one (Set 3)? In each set there is a key, which on being pressed down, sounds a buzzer. This key is the correct key for that particular set, and, of course, in any given set there is but one key which will sound the buzzer. Now in all of these settings, and I have more, there is a common factor which determines which will be the correct key. In other words, you can work out a rule which will tell you which key will be correct for any setting I may give you. Is this clear? (*E* repeats material of directions if necessary.)

Your task is to work out a rule in as few 'sets' as you possibly can. Push down whatever keys you wish, while you look for the correct key. Analyze the situation as you go along. Think aloud. Take your time. Do not rush. I shall give you as many sets as you want. Tell me when you are ready for a new one. As soon as you have a rule to offer, announce it at once, and I shall then give you some sets

to test it out. If your rule works, you have solved the problem. Remember to watch carefully for the critical factor determining which is the correct key for each set, and to think aloud.

These directions were repeated if necessary. After the completion of the first problem, *S* was given the additional instructions:

You have just solved one problem. Shall I reread your directions? Every problem will be presented in the same manner as the one you have just finished. Each, however, depends on a different factor for its solution. Do not forget that. Remember that it is important that you think aloud, and that you announce your solution to me just as soon as you can.

Beginning with Problem IV, the *S*s were allowed to use notes, but they were told that this privilege did not signifiy that the problems were getting more difficult but that they were merely allowed this aid as some *S*s had complained that "solving a problem was just a matter of a good memory." "Note taking" was to be construed merely as a convenience to the *S*, and to be done in any manner he desired, or not at all, if he so saw fit. The same directions given at the beginning still held.

The "thinking aloud" enabled *E* to keep in close touch with *S*'s progress. If *S* found it difficult or distracting, it was not insisted upon. The privilege of taking notes on the latter problems (Problems IV, V, and VI) makes the situation less artificial, and helps to eliminate the objection made by many *S*s during the preliminary experiments, viz. that solving the problems was a matter of memory.

(C) Experimental conditions. The experimental work was conducted in a quiet room. Preliminary work with 20 *S*s was done during the school year 1928-1929. The results of these experiments are not included, however, in this report. The results here reported were obtained during the years 1929-1931 from 60 *S*s, of whom 49 were undergraduate students, 6 graduate students *(KS, JC, TR, MeB, AG, MP)*, 2 members of the staff *(EMT, MM)*, 2 young school teachers *(IK, LG)*, and 1 commercial student *(CS)*. The sexes were equally divided. The ages of the *S*s varied from 16-30 yr.; most (50), however, being between 17-22 yr. All were naïve, at the outset of the experiment, in this kind of work.

As has been indicated, there were two card periods, each separated by intervals of approximately 48 hr. The average time necessary for the solution of the problems was roughly 2 hr. In view of fatigue, the *S*s were not allowed to work more than an hour at a time. Because of individual differences in the rate of working, no rigid rule could be followed regarding the number of multiple choice problems to be completed in one experimental sitting. In general, however, Problems I, II, III, and IV were solved in the first sitting, and V and VI in the last.

(D) Method of scoring. (1) Cards. Correct cards were counted until more than three successive errors were made. Errors were of name, suite, and omission.

(2) Multiple choice. Scoring was done in terms of the number of 'sets' necessary until an adequate solution was formulated.[10] The scores represent the difference between this value and the number of sets necessary to illustrate the problem.

[10] The number of 'sets' has been used rather than the number of 'errors' (moves): (1) because of the emphasis on the set as a situation; and (2) because of the complications in Problem IV, where on the basis of 'errors,' an *S* going through 40 sets might receive a better score than another *S* who experimented with various keys finally getting the problem in 20 sets.

TABLE II

RAW SCORES FOR the DIFFERENT Ss in the DIFFERENT PROBLEMS

S	Multiple Choice						Cards	
	I	II	III	IV	V	VI	1	2
MM	1	1	1	3	2	2	17	31
IK	0	1	2	4	4	2	15	34
RA	1	1	1	7	8	23	20	30
KS	2	1	2	4	1	NC	12	10
HF	1	1	7	3	9	19	6	9
EMT	1	1	1	37	1	3	11	14
FR	3	1	2	7	23	23	11	16
JO	1	1	1	9	37	3	11	29
LL	3	1	2	26	28	14	21	30
CF	1	2	2	38	0	NC	8	12
MeB	0	0	4	25	NC	4	11	14
BZ	1	1	11	29	10	35	18	31
BC	1	16	3	23	26	9	14	24
Le	1	1	8	45	39	3	24	35
LS	1	1	4	DNC	19	5	27	35
RN	1	1	4	DNC	30	3	19	38
ML	1	DNC	4	DNC	38	31	12	32
LA	1	1	4	7	9	DNC	12	14
ET	1	2	3	17	2	DNC	13	33
ES	2	1	8	4	15	DNC	7	15
FG	2	1	4	3	27	DNC	15	28
AG	1	1	1	43	1	DNC	12	9
RP	5	0	1	11	39	DNC	17	29
IB	0	2	1	8	39	DNC	13	37
EvB	4	2	3	29	27	DNC	10	18
RC	1	13	3	23	NC	DNC	6	10
BS	1	10	2	DNC	DNC	1	13	13
SF	1	DNC	DNC	3	DNC	4	8	13
JR	2	1	DNC	DNC	DNC	40	13	27
NR	3	2	13	DNC	24	DNC	4	5
AF	1	1	2	DNC	NC	DNC	12	28
DR	1	2	6	DNC	NC	DNC	11	14
EK	1	5	19	DNC	NC	DNC	15	10
MP	1	1	2	1	DNC	DNC	16	20
HH	3	2	2	5	DNC	DNC	14	19
JS	1	2	5	1	DNC	DNC	11	16
LG	1	2	3	14	DNC	DNC	12	16
TR	1	3	3	15	DNC	DNC	22	32
VK	1	1	7	15	DNC	DNC	15	17
WL	1	2	13	10	DNC	DNC	16	21
SJ	1	1	2	38	DNC	DNC	16	13
SS	2	15	10	6	DNC	DNC	20	32
EJ	6	3	19	16	DNC	DNC	7	11
BK	2	1	DNC	DNC	7	DNC	12	15
EmB	1	1	DNC	DNC	9	DNC	8	18
AW	5	1	DNC	DNC	21	DNC	22	29
DB	6	DNC	11	38	DNC	DNC	18	24
SG	2	1	DNC	32	DNC	DNC	9	15
RH	1	0	1	DNC	DNC	DNC	11	18
CL	1	1	1	DNC	DNC	DNC	26	49
EL	1	0	5	DNC	DNC	DNC	7	15
SK	6	2	3	DNC	DNC	DNC	7	11
EdL	1	7	7	DNC	DNC	DNC	16	29
PJ	1	1	18	DNC	DNC	DNC	25	37
CS	1	14	4	DNC	DNC	DNC	4	9
MaB	1	9	10	DNC	DNC	DNC	15	15
EF	1	DNC	4	DNC	DNC	DNC	9	17
GK	2	2	DNC	DNC	DNC	DNC	11	17
AC	3	2	DNC	DNC	DNC	DNC	9	15
LR	2	18	DNC	DNC	DNC	DNC	13	16
Mean	1.70	2.98	5.08	16.6	14.9	12.5	13.5	21.3

These subtraction values are as follows: Problem I, 1; II, 2; III, 1; IV, 2; V, 2; and VI, 5.

Unknown to *S*, the number of sets to be given for each problem was limited: Problems I, II, and III to 20 sets; and Problems IV, V, and VI to 45 sets. After the allotted number of sets had been used by an *S* (without resulting solution), he

was given a DNC unless his "thinking aloud" seemed to indicate that a solution was near. In this event, he was given several more sets (usually 10). In all cases but one *(Le)* these extra sets did not result in a solution. The actual solutions were divulged indirectly to *S*s receiving DNC by means of pointers, and special sets, in such a way that they would think themselves responsible for most of the solution.

NC represents an adequate but inferior solution. These will be discussed later in greater detail.

(E) Method of treating results. (1) Cards. Inasmuch as Period 1 with the cards may be considered a period of practice in method for the *S*, the results for Period 2 alone have been used. This second period is complicated by the inclusion of a row of Hearts (A, 8, Q, 6) in the center of the deck, to serve as an objective and incentive. Scores, therefore, have been ranked.

(2) Multiple choice. The problem of DNC and NC has been attacked in two ways. For intercorrelations, for which the results of individual problems are used, scores have been given rank orders. DNC and NC, hence, receive the median rank of their group. At best this method is somewhat gross, as can be seen from the fact that an *S* with a score of two receives a rank of 39, and a rank of 18.5 for a score of one (Problem II). The results of Problem I have not been used for the reason that they show but little scatter, and hence, have low value as a comparatively sensitive measure of individual differences. Moreover, Problem I serves as an introductory problem. Obviously, these individual problem rankings are inadequate for combination into battery form, for all or any portion of the total number of problems. Further difficulty is encountered in using these individual ranks in dealing with the problem of DNC. A DNC for Problem III receives a rank of 56, for Problem VI, a rank of 40.5. It is thus possible for an *S* to receive a 'worse' total ranking by missing an 'easier' problem and solving the latter problems, than by completing the earlier problems and not completing the latter ones.

It therefore becomes necessary to use some method of weighting based upon some criterion of difficulty, in handling these DNC. The use of the mean does not seem feasible with these data. An inspection of the means for each problem reveals that the mean reaches a peak in Problem IV, and then diminishes, whereas the number of DNC does not keep pace. The following shows this in unambiguous terms:

Problem	I	II	III	IV	V	VI
Mean	1.70	2.98	5.08	16.6	14.9	12.5
No. DNC	0	4	9	24	27	40

The number of DNC for Problem VI is almost double that for Problem IV, although the mean for Problem VI is much lower. In this connection, it should be stated that DNC and NC do not enter into the computation of these means. The means for the latter problems are based on fewer cases as a consequence.

The criterion of difficulty chosen was the number of *S*s completing the given problem. With 56 as a base (Problem II), the weighting indices are as follows:

Problem	II	III	IV	V	VI
Weighting Index	1.000	1.098	1.555	1.697	2.800
Reciprocal	1.0000	.9107	.6429	.5893	.3571

Treating the scores now becomes a matter of evaluating *S*'s performance on the basis of (a) the problems completed, and (b) the score obtained in them. On

the basis of (a) an *S*'s performance will fall into one of 15 groups on the basis of summation of weighting indices for those problems he completes. Thus an *S* completing all the problems will fall in the first ranking group (Table III) with a summative ranking index of 8.15. The position of this *S* within the group will depend upon his performance in the individual problems (b). These scores, in

TABLE III

SUMMATIVE RANKING INDICES IN TERMS OF PROBLEMS COMPLETED

Problem						Battery	
II	III	IV	V	VI	f	A	B
					14	5.350	8.150
		+			2	3.795	6.595
+		+			1	2.795	5.595
				+	9	5.350	5.350
		+	+		1	2.098	4.898
+	+		+		1	1.555	4.355
	+	+	+		1	1.000	3.800
		+		+	4	3.795	3.795
			+	+	10	3.653	3.653
	+	+		+	3	2.697	2.697
+			+	+	1	2.653	2.653
	+		+	+	1	2.555	2.555
		+	+	+	8	2.098	2.098
+		+	+	+	1	1.098	1.098
	+	+	+	+	3	1.000	1.000

terms of sets, are equated by the use of the reciprocal of the individual problem indices, and summed. By means of these two steps it is possible to rank the *S*s in a very clearly differentiated manner. In only one instance do 2 *S*s receive the same ranking (*GK* and *AC*).

Table III gives the summative ranking indices and *S* frequencies in terms of problems completed. Blank spaces indicate that the problem was solved; plus signs represent DNC. For reasons given earlier, the results for Problem I are not used. Because of the large number of DNC for Problem VI, the summative ranking indices are given for two batteries—A and B. Battery A includes Problems II, III, IV, V; and B, Problems II, III, IV, V, and VI.

TABLE IV

DNC DISTRIBUTION IN PROBLEMS

Problems	II	III	IV	V	VI
II	(4)				
III	1	(9)			
IV	2	7	(24)		
V	3	6	14	(27)	
VI	2	7	19	24	(40)

The distribution of the DNC for the various problems is given in Table IV. The parenthetical number represents the total number of DNC for the corresponding problem number listed at the head (and side) of the column. The distribution is given by the other values listed, for example (line 2 of table): of the four *S*s receiving DNC in Problem II, one did not complete Problem III,

TABLE V

RANKINGS FOR THE DIFFERENT Ss IN THE DIFFERENT PROBLEMS

	Multiple Choice						Battery		Cards
S	I	II	III	IV	V	VI	A	B	2
MM	22.0	18.5	5.0	4.5	5.5	2.5	1.0	1.0	12.5
IK	2.0	18.5	14.5	8.0	7.0	2.5	3.0	2.0	7.0
RA	22.0	18.5	5.0	13.0	9.0	14.5	4.0	3.0	14.5
KS	45.0	18.5	14.5	8.0	3.0	19.5	2.0	4.0	55.0
HF	22.0	18.5	39.0	4.5	11.0	13.0	6.0	5.0	58.0
EMT	22.0	18.5	5.0	31.0	3.0	5.5	11.0	6.0	46.5
FR	52.0	18.5	14.5	13.0	17.0	14.5	9.0	7.0	36.5
JO	22.0	18.5	5.0	16.0	24.0	5.5	13.0	8.0	17.5
LL	52.0	18.5	14.5	27.0	22.0	12.0	18.0	9.0	14.5
CF	22.0	39.0	14.5	33.0	1.0	19.5	12.0	10.0	51.0
MeB	2.0	2.5	30.5	26.0	31.0	8.5	20.0	11.0	46.5
BZ	22.0	18.5	45.5	28.5	13.0	17.0	17.0	12.0	12.5
BC	22.0	55.0	23.0	24.5	19.0	11.0	21.0	13.0	23.5
Le	22.0	18.5	41.5	36.0	27.0	5.5	23.0	14.0	5.5
LS	22.0	18.5	30.5	48.5	15.0	10.0	26.0	15.0	5.5
RN	22.0	18.5	30.5	48.5	23.0	5.5	28.0	16.0	2.0
ML	22.0	58.5	30.5	48.5	28.0	16.0	40.0	17.0	10.0
LA	22.0	18.5	30.5	13.0	11.0	40.5	5.0	18.0	46.5
ET	22.0	30.0	23.0	23.0	5.5	40.5	7.0	19.0	8.0
ES	45.0	18.5	41.5	8.0	14.0	40.5	8.0	20.0	41.5
FG	45.0	18.5	30.5	4.5	20.5	40.5	10.0	21.0	20.5
AG	22.0	18.5	5.0	35.0	3.0	40.5	14.0	22.0	58.0
RP	56.5	2.5	5.0	18.0	27.0	40.5	15.0	23.0	17.5
IB	2.0	39.0	5.0	15.0	27.0	40.5	16.0	24.0	3.5
ErB	55.0	39.0	23.0	28.5	20.5	80.5	19.0	25.0	29.5
RC	22.0	52.0	23.0	24.5	31.0	40.5	22.0	26.0	55.0
BS	22.0	51.0	14.5	48.5	47.0	41.0	50.0	27.0	49.5
SF	22.0	58.5	56.0	4.5	47.0	8.5	55.0	28.0	49.5
JR	45.0	18.5	56.0	48.5	47.0	8.0	57.0	29.0	22.0
NR	52.0	39.0	47.5	48.5	18.0	10.5	29.0	30.0	60.0
AF	22.0	18.5	14.5	48.5	31.0	40.5	24.5	31.0	20.5
DR	22.0	39.0	37.0	48.5	31.0	40.5	27.0	32.0	46.5
EK	22.0	48.0	50.5	48.5	31.0	40.5	20.0	33.0	55.0
MP	22.0	18.5	14.5	1.5	47.0	40.5	30.0	34.0	26.0
HH	52.0	39.0	14.5	10.0	47.0	40.5	31.0	35.0	27.0
JS	22.0	39.0	35.5	1.5	47.0	40.5	32.0	36.0	36.5
LG	22.0	39.0	23.0	19.0	47.0	40.5	33.0	37.0	36.5
TR	22.0	46.5	23.0	20.5	47.0	40.5	34.0	38.0	10.0
VK	22.0	18.5	39.0	20.5	47.0	40.5	35.0	39.0	33.0
WL	22.0	39.0	47.5	17.0	47.0	40.5	36.0	40.0	25.0
SJ	22.0	18.5	14.5	33.0	47.0	40.5	37.0	41.0	29.5
SS	45.0	54.0	43.5	11.0	47.0	40.5	38.0	42.0	10.0
EJ	59.0	46.5	50.5	22.0	47.0	40.5	39.0	43.0	52.0
BK	45.0	18.5	56.0	48.5	8.0	40.5	41.0	44.0	41.5
EmB	22.0	18.5	56.0	48.5	11.0	40.5	42.0	45.0	29.5
AW	56.5	18.5	56.0	48.5	16.0	40.5	43.0	46.0	17.5
DB	59.0	58.5	45.5	33.0	47.0	40.5	44.0	47.0	23.5
SG	45.0	18.5	56.0	30.0	47.0	40.5	45.0	48.0	41.5
RH	22.0	2.5	5.0	48.5	47.0	40.5	46.0	49.0	29.5
CL	22.0	18.5	5.0	48.5	47.0	40.5	47.0	50.0	1 0
EL	22.0	2.5	35.5	48.5	47.0	40.5	48.0	51.0	31.5
SK	59.0	39.0	23.0	48.5	47.0	40.5	49.0	52.0	52.5
EdL	22.0	49.0	39.0	48.5	47.0	40.5	51.0	53.0	17.5
PJ	22.0	18.5	49.0	48.5	47.0	40.5	52.0	54.0	3.5
CS	22.0	53.0	30.5	48.5	47.0	40.5	53.0	55.0	58.0
MaB	22.0	52.0	43.5	48.5	47.0	40.5	54.0	56.0	41.5
EF	22.0	58.5	30.5	48.5	47.0	40.5	56.0	57.0	33.0
CK	45.0	39.0	56.0	48.5	47.0	40.5	58.5	58.5	33.0
AG	52.0	39.0	56.0	48.5	47.0	40.5	58.5	58.5	41.5
LR	45.0	56.0	56.0	48.5	47.0	40.5	60.0	60.0	36.5

two did not complete Problem IV, three did not complete Problem V, and two did not complete Problem VI. It will be noted that in all cases but two, there is an increase in the number of DNC as we go from the easier to the more difficult problems. This, too, is in accord with our criterion. The two inversions are minimal.

In Table V will be found the raw data converted to ranks. This method of combining the scores has several advantages. The weighting index is not arbi-

trarily fixed, but varies directly with the actual results. One may consider 56 points to be available for each problem solved, the number of *S*s solving a given problem determining the 'credit' to be given each *S* for that problem. The number of *S*s solving a given problem will vary with the number of DNC for that problem. Yet, it will not be necessary to deal directly with the DNC in treating the data, either by way of assigning arbitrary scores to them, using a method of ranking, or percentiling. In addition, our method can be used conveniently to combine any problems in the battery, so that one need not depend on the efficacy of partials in working out the effect of each problem on the battery. A further advantage of this method is that within each ranking group, an *S* is compared only with other *S*s who have successfully completed the same problems as he.

NC have been regarded as successful solutions, in determining the summative rank index of the *S* under consideration. In computing the *S*'s rank within the rank group, NC were given the maximum number of sets possible for that problem. As has been stated earlier, these NC cases represent an adequate but 'inferior' solution. For example, one *S* (*EK*) formulated the following as the solution of Problem V: "When there are 3 keys, it's the first; 5 keys, the second; 7 keys, the third; 9 keys, the fourth; 11 keys, the fifth. Each time you count from the opposite side that you counted from before." The solutions offered by the other 4 NC for this problem are similar to *EK*'s. Two *NC* will be found for Problem VI (*KS* and *CF*). In both instances a solution was offered, "when there's 1 group, it's the first key; 2 groups, the second key." Nothing was offered for 3 and 4 groups, although the correct key was found in every instance without error. The NC total 7 in number and fall in Problems V and VI only.

The question arises—can these final scores in terms of rank, be regarded as scores? Strictly speaking, the answer is no. The most telling criticism of ranking is that differences in rank and in score are not commensurate. Our data, however, reveals comparative continuity both in respect to differences in the same summative rank group, and differences between groups. In any given group the differences from *S* to *S*, for the highest part, are both clean cut and comparatively regular. The differences from group to group are not great when considered in terms of the frequencies of *S*s falling into the various groups. It seems to the writer that the limitations usually imposed by a method of ranking are at a minimum in his treatment, and that in a looser sense, although a comparatively accurate one, these ranks may be construed as scores.

The foundation of our method of treating the multiple choice results is the criterion chosen for difficulty. A consequence of this criterion is that an *S*'s rank is determined primarily by the problems he completes, and only secondarily by his proficiency (in terms of sets necessary for solution) in these problems. As a result, no matter how well an *S* may acquit himself in the problems he solves, a second *S*—who may require the maximum number of sets, but who on the other hand solves either more problems or an equal number of greater "weight"—will receive a higher rank than the first. This consideration presents a great difficulty to any plan for converting the *S*'s complete performance to score form. We have tried several other methods of treating the results, but have been unable to overcome this difficulty without losing the advantages gained by this ranking technique. Among these other methods have been: (a) using the total number of sets possible, as a base for weighting indices (the proportionality of each index to the

others is not changed), adding the reciprocals to the summed equated values of individual problem scores; (b) using a value derived by dividing the total number of sets possible by the summative rank index, and adding the equated scores to this value as a base; and (c) adding the maximum number of sets for each problem not solved to the *S*'s summed equated scores. All these methods gave scores to some *S*s which were at variance with his criterion of difficulty. Using larger although still proportional (with reference to the criterion) gaps between ranking groups, introduces factors not dependent on the data themselves. An artificial discontinuity is introduced between *S*s falling in one group and *S*s falling into an adjacent group. It should be remembered that these groups consist of *S*s who have solved the same problems. These groups may be considered as the 15 performance patterns made by the *S*s in solving the problems. These 'patterns' are in no way dependent upon any criteria of difficulty.

It seems reasonable to assume that the data has continuity, and that the continuity between groups (obtained by our ranking procedure) is more nearly adequate than the discontinuity between groups attendant on the introduction of a "gap" arbitrarily made large enough to prevent overlapping between groups, as is necessary in accordance with our criterion.

RESULTS

(1) *Cards.* An examination of the gross results (Table II) indicates an average improvement of 57.8% for Period 2 over Period 1, with an S.D. of 2.68 for Period 1, and 4.74 for Period 2. All *S*s but 5 (*KS, AG, BS, EK, MaB*) improved in the second period. Of these *S*s, 3 showed losses. Improvement in the remaining 55 *S*s varies from 1-24 cards. The average improvement for all cases is 7.8 cards. In terms of the standard errors of the means, the reliability of the difference is expressed by $7.8 \pm .703$. Period 2 correlates $0.777 \pm .034$ with Period 1 (product-moment).

The method seems adequate to our purpose. The range is large, much larger than with most 'learning methods' currently used. It did not prove necessary to teach the names of the cards to any *S*. The conditions imposed upon *S* in learning the cards, lessen the artificiality of the laboratory conditions usually imposed. The conditions more nearly approximate those of his everyday learnings in that the *S* is free to set his own pace.

(2) *Multiple choice.* In view of the diversity of 'principles' in the various problems, it will be interesting to work out the intercorrelations of problems of the battery. These values will be found in Table VI. Individual problem rankings have been used in the computations. The limitations of these values have been discussed above on page 423. These correlations are, therefore, somewhat gross and give us little more than trends.

How valuable is each problem to the battery? We have gone about answering this question in two ways: (a) the correlation of each problem

TABLE VI

INTERCORRELATIONS OF PROBLEMS

Problems	II	III	IV	V
III	.33 ± .078			
IV	.20 ± .084	.34 ± .077		
V	.44 ± .070	.34 ± .077	.30 ± .079	
VI	.37 ± .075	.39 ± .074	.35 ± .076	.55 ± .061

with the battery; (b) the effect on the battery when the individual problem is omitted.

(a) The correlation of each problem with the battery,[11] both when A and B are used, is given in Table VII.

(b) With the method of battery combination outlined above (p. 424), it is possible to combine any of the problems used. Hence, we can correlate a battery consisting of Problems III, IV, V, VI, with a battery consisting of Problems II, III, IV, V, VI; a battery of Problems III, IV, V with II, III, IV, V and find the effect of Problem II, and so on. These values are given in Table VIII. Battery A consists of Problems II, III, IV, V; and B, of Problems II, III, IV, V, VI. Parentheses indicate omission, hence (II) under A designates that this correlation value is of Problems III, IV, V with a battery consisting of Problems, II, III, IV, V, and so on.

TABLE VII

CORRELATIONS OF EACH PROBLEM WITH BATTERIES

Problem	Battery	
	A	B
II	.45 ± .069	.39 ± .074
III	.57 ± .059	.53 ± .063
IV	.65 ± .050	.56 ± .060
V	.81 ± .030	.78 ± .034
VI	.48 ± .067	.78 ± .034

TABLE VIII

CORRELATIONS TO SHOW THE EFFECT OF PROBLEM ON BATTERY WHEN PROBLEM IN QUESTION IS OMITTED

Problem omitted	Battery	
	A	B
(II)	.99 ± .002	.99 ± .002
(III)	.96 ± .007	.97 ± .005
(IV)	.92 ± .013	.94 ± .010
(V)	.79 ± .033	.90 ± .017
(VI)		.87 ± .021

[11] All correlations, except as otherwise stated, have been done by the method of Rank Differences: $\varrho = 1 - 6\Sigma(R_x - R_y)^2/N(N^2 - 1)$; $r = 2 \sin[(\pi/6)\varrho]$.

From Table VII it will be seen that, when the entire battery (B) is considered, Problems V and VI correlate rather highly and equally with the battery. Moreover, except in the case of Problem VI, the correlations in Batteries A and B vary but little. The greater difference in the case of Problem VI (0.48 and 0.78) may be due to the fact that the effect of Problem VI is not felt in Battery A. The scores of individual problems were taken care of by rankings, in the computations of these correlations.

In determining the effect of individual problems on the battery by means of (b) the procedure is somewhat indirect. When a particular problem is omitted from a battery, the greater the correlation with the complete battery, the less the effect (or disturbance) may be said to be due to this problem, and hence, the less the problem under consideration contributes to the battery. Table VIII clearly reveals that Problem II contributes least, and Problem VI the most (when Battery B is considered). The intermediate differences are not great, however. The equivalent values for Battery A vary but slightly from those for B, except in the instance of Problem V.

It would seem, from an inspection of Tables VII and VIII, that the battery of problems given the *S* might be reduced without suffering especially in efficacy. Some of the possible combinations of problems and their correlations with Batteries A and B are given in Table IX. The best combination for both batteries would seem to be a combination of III, IV and V; with IV and V almost as efficient. These combinations should be verified experimentally, since the results of our preliminary experiment give evidence that the "order of presentation" is a factor in their difficulty to the *S*.[12]

(3) *Correlations between cards and multiple choice problems.* The correlation between the results in card learning and Battery A is 0.08 $\pm$ .087, and between card learning and Battery B is 0.10 $\pm$ .086. The correlation between card learning and the different multiple choice problems is: Problem II, 0.23 $\pm$ .083; III, 0.19 $\pm$ .084; IV, 0.09 $\pm$ .086; V, 0.07 $\pm$ .087; and VI, 0.30 $\pm$ .079.

The correlations for the batteries are very low. The correlation of 0.30 of Problem VI with the cards, is based upon data in which 67% of the *S*s (40) receive the same ranking. It is curious that the correlations of individual problems are lowest for Problems IV and V whose results give

[12] See also W. Brown and F. Whittell, Yerkes' multiple choice method with human adults, *J. Comp. Psychol.*, 3, 1923, 305-318, esp. 315-316.

the greatest scatter. These correlations (.09 and .07) are virtually identical with the battery correlations (.08 and .10), although these two problems do not correlate to a commensurate extent with the two batteries (see Tables VII and VIII). The probable errors are all high, and further reduce the minimal significance of these correlations.

TABLE IX

CORRELATIONS OF VARIOUS PROBLEM COMBINATIONS WITH BATTERIES A AND B

Problems	Battery	
	A	B
II, III, IV	.79 ± .033	.64 ± .051
II, III, V	.92 ± .013	.84 ± .026
II, IV, V	.96 ± .007	.87 ± .021
III, IV, V	.99 ± .002	.91 ± .015
IV, V, VI	.83 ± .027	.96 ± .007
IV, V	.94 ± .010	.89 ± .018

(4) *Sex differences.* (a) *Cards.* The data when classified according to sexes reveal an average of 24.4 correct cards for men, and 18.2 for women (Period 2). A proportionate difference is found when the results for Period 1 are considered: 15.5 correct cards for men and 11.1 for women.

(b) *Multiple choice.* These results when analyzed as to sexes, also give the men general superiority. In Table X are listed the average number of sets necessary for solution; the number of *S*s on which these averages are based—the number of *S*s successfully completing the respective problems; the number of DNC; and the number of NC. Neither DNC nor NC enters into the computation of these averages.

TABLE X

SEX DIFFERENCES WITH PROBLEMS

	Problems									
	II		III		IV		V		VI	
	M	W	M	W	M	W	M	W	M	W
Av. no. sets	2.03	4.00	4.26	6.00	20.3	12.6	16.6	19.3	7.4	20.4
No. completing	29	27	27	24	19	17	18	10	11	7
No. of DNC	1	3	3	6	11	13	9	18	17	23
No. of NC	0	0	0	0	0	0	3	2	2	0

It will be noted that, with the exception of Problem IV, the men are definitely superior to the women both from a standpoint of the average number of sets, and the number of DNC. In the aggregate there are 41 DNC for men to 63 for women. In Problem IV alone, do the females prove superior to the males, although the DNC are almost equally distributed.

These sex differences may be considered to be summed up by the sex distribution in the batteries: In Battery B, of the first 30 ranks, 17 are occupied by men (av. 11.5), and 13 by women (av. 20.7). For the entire battery the average rank of the men is 26.17, and of the women, 34.83. The difference is even more marked in Battery A. Here, of the first 30 ranks 19 are occupied by men (av. 14.5), and 11 by women (av. 17.3). The average rank for this entire battery is 25.7 for men, and 35.7 for women.

The method of 'standard error' does not seem an adequate instrument in determining the reliability of the differences of the respective averages of men and women. Our experimental technique has placed a premium on scatter. The data, for the greatest part, show well distributed scores which do not fall into a distribution even analagous to a Gaussian distribution. Our statement of definite sex differences is based on three lines of evidence: (1) respective number of DNC for men and women (multiple choice); (2) the respective averages (multiple choice and cards); and (3) the rank distributions in the batteries and cards. The matter of the possibility of sampling error will be further discussed in a later paragraph.

In view of these sex differences it may be of advantage to correlate the cards with multiple choice, when the sexes are taken separately. These correlations have been computed by correlating the respective ranks of cards and multiple choice, by the product-moment method.

Sex	M	W
Battery A	$-.175 \pm .119$	$.181 \pm .119$
Battery B	$-.123 \pm .121$	$.275 \pm .114$

These correlations, while not high, are suggestive in spite of the high P.E. It will be remembered that Problem VI correlates highest of the individual problems with the cards; and that, of the 20 *Ss* completing this problem, 17 were men. We should expect, therefore, an accentuation of the r for the men, when Problem VI is omitted (Battery A). It is evident that such has been the case.

From these results on sex differences it seems highly possible that there may be present definite correlation trends between serial learning and generalizing abstraction when the sexes are treated separately. These trends will be obscured when the results are taken in the aggregate. Our results, however, are based on too few *Ss* to make a positive statement.

We have the following evidence to submit that these sex differences are *not* due to the sampling of *Ss* used in the investigation: (1) Sampling would not

account for the superiority of the women with Problem IV. (2) A sampling error seems incompatible with the negative correlation trend of Cards with Multiple Choice for men, and with the higher positive trend of correlation for women. (3) S.A.T. scores (College Entrance Board) for 42 of the 60 *S*s (24 women and 18 men) give the 'usual' distribution of the sexes: men, av. 482.1; women, av. 479.3. This item, while not inclusive, seems indicative.

An examination of the nature of Problem IV may give us a few suggestions regarding the respective performance of men and women. In this problem, the principle is 'odd-even,' with the right end of the set as the point of reference. In solving this problem, most *S*s went through these steps: (1) found point of reference, (2) attempted to work out a sequence to predict the appearance of the first and second keys as the correct key, (3) failing in this, changed method of attack. All DNC went through the first two steps listed. Except for those few *S*s completing the problem within very few sets, much of the difference in scores may be accounted for in terms of the persistence of the sequence attitude, in spite of the contradictions given the *S* by his own data. There exist, consequently, the possibilities that the sex differences in the performance of this problem may be due to 'persistence' differences between the sexes, or, to a feminine aptitude for odd-even relationships.

Summary and Conclusions

A study was made of serial learning and generalizing abstraction, as revealed by the learning of the serial order of a deck of playing cards and the Yerkes multiple choice method respectively. The results on 60 *S*s show:

(1) A non-significant, although positive correlation between serial learning results and generalizing abstraction results: $0.08 \pm .087$; $0.10 \pm .086$.

(2) Sex differences in both serial learning and generalizing abstraction. These differences do not seem entirely accountable for in terms of a sampling error.

(3) Correlations between serial learning and generalizing abstraction results, when the sexes are treated separately, are positive for women ($0.181 \pm .119$; $0.275 \pm .114$); and negative for men (-0.175 ± 119; $-0.123 \pm .121$). There are too few cases, and the P.E. are quite high. The values are greater, however, than those obtained when the results are taken irrespective of sex, and seem extremely suggestive.

CONFIGURATIONAL TENDENCIES IN RORSCHACH RESPONSES

By SAMUEL J. BECK, Boston Psychopathic Hospital

One of the principal diagnostic indices in the Rorschach technique for personality probing is the *W*, or *whole*, response (Rorschach's *G*, or *Ganz*). As the term indicates, this type of response is one in which the subject reacts and gives meaningful interpretation to the entire ink-blot figure, rather than details of it. There are 10 ink-blot figures in the series composing the test. Of the whole response Rorschach says:

"Die Zahl der G ist vor allem als Indikator der dispositionellen Energie des Assoziationsbetriebs zu betrachten, oft auch als Indikator des bewussten oder unbewussten Willens zur komplizierteren Leistung (Abstraktions- oder Kombinationsleistung: grosse Zahlen guter, primärer G haben oft philosophisch Veranlagte, grosse Zahlen guter, kombinatorischer G produzieren häufig Phantasiebegabte). Ein Optimum dieser Fähigkeiten ist eine weitere Komponente der Intelligenz."[1]

His table of norms shows the incidence of *W* at various levels of intelligence to be as follows:[2]

Superior	10 or more *W*s
High average	7–10 *W*s
Average	4–7 *W*s
Low average	3–4 *W*s
Moron	1–3 *W*s
Imbecile	0–2 *W*s

It would follow from this discussion and these norms that wholes (*W*s) are a function of intelligence. The writer's experience with the test in a number of groups has, however, clearly indicated a necessary qualification of this principle. For it becomes quickly manifest that some of the ink-blot figures are much more frequently interpreted as wholes (*W*s) than others. Thus the fifth in the series is almost always seen as a whole (*W*); the fourth and first very frequently. The tenth on the other hand is almost never interpreted as a whole (*W*), and the ninth and eighth with great rarity. If the whole (*W*) response is an indicator of intelligence, is it equally valuable as such an indicator in figures yielding such unequal number of wholes (*W*s)? It is possible, in fact, that in the fifth figure the *W* is a sign of lack of, or lack of functioning of, intelligence, on the theory that it is most easily interpretable as a *W*, and that breaking it up into details is an index to a heightened intellectual activity, capable of breaking the stimulus up into meaningful parts. In a word, frequency of *W*s in the respective cards varied. The blots vary in configuration, shading, and color. Presumably the difference in frequency of *W*s is related to these variations. To the extent that the whole re-

* Accepted for publication March 9, 1932.

[1] H. Rorschach, *Psychodiagnostik*, 1921, 49.

[2] *Ibid.*, 40 f. Rorschach's terms are, respectively: "Künstler, Intelligente, Durchschnitt, Unintelligente, Debile, Imbezile." He does not state the classifications to have been made on the basis of any psychometric instrument.

sponse is based only on configuration the notion of its value as indicator of intelligence is in fact susceptible to *reductio ad absurdum*. For, let us assume, as one of the figures, a solid black square. All responses to it would very likely be *W*s. Would they still be an indicator of intelligence?

If, in other words, frequency is an index to difficulty value, and difficulty value is an index to level of intellectual functioning, then the value of *W*, as indicator of height of associational energy, varies in the respective figures. That is, the value of *W* as indicator of associative energy, varies with the conditions under which *W* is obtained, the variation in these conditions being represented by the variation in configuration, shading, and color in the respective figures. What, then, is the *W* value for each figure? As value varies with the conditions, what light is thrown on the fluctuation of this psychological process, associative energy—or more accurately, organizing energy[a]—with change in conditions? In examining the material forming the basis of this report, quantitative findings were available making possible some tentative conclusions on this 'organizing energy,' not only in so far as it issues in production of whole responses, but also in associating various details into larger, meaningful combinations. These organized combinations fall into fairly clear-cut groups. Thus the combination may consist of (a) adjacent details; (b) distant details, separated either by white space or by solid portions; (c) white spaces combined with adjacent solid portions; (d) portions of the figure were broken up, and the elements so obtained recombined into meaningful percepts, an analysis-synthesis process; (e) whole, *W*, responses; (f) elements of the figure were successively seen in meaningful relation to one another, until the entire figure was included in the response, but the reaction was at no time to the whole of the figure; (g) what may be described as a "striving towards a whole," without it being achieved, as when one *S*, reacting to the tenth figure, said: "The drawing doesn't seem to be held together; they [the details of it] are scattered; they give you a feeling of being thrown together more than put there." Another *S*, in responding to the ninth figure complained of its "lack of integration." Incidence of the combinations classified under the first five headings was sufficiently high to make possible statistical treatment. The other two groups were represented by too few for such treatment. Examples of the respective groups appear on the page opposite.

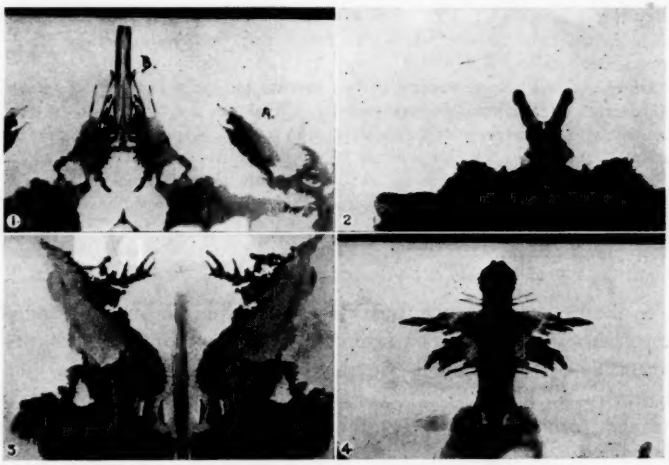
Fig. 1 reproduces a portion of the tenth Rorschach figure. The response was: "These green figures (A) are like beetles, or scarabs, . . . worshipping a central figure (B); they seem to be about to fall down; to prostrate themselves." Thus two details which are rarely associated with each other were seen in relation by this *S*. The mental act included a leap across an intervening white space. The example is cited as illustrating the association of distant details.

This response is also interesting in that it seems to illustrate processes emphasized by the *Gestalt* group. Thus Detail A was not seen alone, nor was Detail B, but the two together have a meaning, in this particular response, not attaching to either of the parts independently but to both as one larger whole. It may be mentioned in passing that many of the organized combinations reported on in this paper offer

[a] Rorschach's language is "Assoziationsbetrieb." It seems clear from the text that "Assoziation" is used in the sense not only of content recalled by the stimulus, *i.e.* the usual sense of the word "association," but also in the sense of associating various details together with reference to one another, *i.e.* organizing them.

evidence of the operation of this *Gestalt* principle. It is not, however, within the scope of the present report to consider this element.

In Fig. 2, is reproduced a portion from the fifth figure in the test series. The response was: "These little things might be two animals kissing each other." This was classified as an association of details adjacent to each other. An example of organization of solid material with white space, both entering into the meaningful interpretation, is illustrated in Fig. 3, taken out of the ninth figure in the series. The response was: "This might be a tulip, or it might be a plant similar to a sensitive plant, and it catches insects," the inner space being the portion where the insects are trapped as the projecting details close. The analysis-synthesis process is



FIGS. 1–4. EXAMPLES OF THE INK-BLOT TEST FIGURES

Fig. 1 is a portion of Rorschach's tenth figure; Fig. 2, of his fifth; Fig. 3, of his ninth; and Fig. 4, of his sixth.

exemplified in the response cited to the portion from the sixth figure in the test series, reproduced in Fig. 4. It "looks like some one stuck up on a crucifix," said *S*, explaining that the lateral flares might be arms sticking out. The inner middle portion which is darker than the flares is the body. Thus the entire detail is first broken up and then recombined. Determination of combinations classified in this group could not be made with the same certainty as for the other groups. Evidence was sought by questioning the *S*s. But this evidence is probably not always reliable. Thus it is possible that in the example cited the reaction was first, to the entire detail, and the apparent analytic process only a rationalization by *S*. The decision as to classifying responses in this group was thus, in a number of cases, made by the examiner on equivocal evidence. To that extent it was a subjective judgment on his part.

Whole responses, as has already been stated, are those in which all elements of the presented material are brought together in one meaningful percept, as "a butterfly," among the most common responses to the first figure. But when a subject, reacting to the same figure, said, "An owl sitting up on a tree," he combined all the elements of the figure without at any time reacting to it as a whole. The middle section of the whole figure was seen as an owl, and the lateral portions as the tree.

The various kinds of organizing processes were not mutually exclusive and *a priori* there is no reason why they should be. Thus a response in the eighth figure of the test series may include a bear stepping over water (white combined with solid), and he is stepping onto a rock, which makes a scene reflected in the portion below (adjacent details). More than one kind of associative process may thus be found in the same response. When so found they were each classified in their respective groups.

Observers. The responses forming the material for the present report were obtained from 39 intellectually very superior individuals: 3 psychologists of senior standing with long records of scientific achievement; 4 psychologists of junior rating; 7 graduate and senior students in psychology from Harvard and Radcliffe; 2 industrial psychologists; 4 social workers; 5 graduate students in social work; 7 physicians, and a social work administrator, a high Federal government official, a mathematician, a statistician, a lawyer, a businessman, and an office secretary. The group proved extremely productive, yielding a total of 2215 responses, with a range from 14 to 139, and a mean of 56.79.

In the present study the usual Rorschach technique of classifying the responses was disregarded in favor of a purely quantitative treatment of organizing acts resulting in meaningful combining of elements in the figures presented to the subject. The procedure consisted in obtaining, for each of his 10 figures, (1) the total number of responses; (2) the total number of organizing acts; and (3) the total number of responses in which more than one organizing act occurred. From these data it was possible to calculate the total number of responses in which no organizing process occurred, as well as the total number of organizing processes which came to expression.

As an example, the findings for the tenth of Rorschach's figure were as follows:

Whole (*W*) responses	19
Combination of distant details	32
Combination of white with solid details	13
Combination of adjacent details	41
Analysis-synthesis processes	3
Combinations into *W* without any one being *W*	4
Total organizing acts	112

However, some of these combinations appeared in the same response, and it was found that there were 106 responses in which one or more associations were affected. There were a total of 331 responses to this figure. We add and subtract as follows:

Total responses	331
Responses in which combinations were found	106
Responses in which there were no combinations, *i.e.* only individual details were reacted to	225
Total organizing acts	112
Mental acts that came to expression	337

It is possible now to obtain a sigma value for each type of associational activity, using the method described by Garrett.[4] Thus in the case of the tenth figure we would get the following values:

	No.	*%*	*Sigma Value*	*Sigma Distance from zero point*
Zero point of associational activity	225	66.7	—.43	
Wholes	19	5.6	1.58 + .43	2.01
Distant details	32	9.5	1.31 + .43	1.74
White with solid	13	3.8	1.77 + .43	2.20
Adjacent details	41	12.1	1.17 + .43	1.60
Analysis-synthesis	3	.09	2.36 + .43	2.79

The same procedure was applied to all 10 figures. The results thus yield the sigma value of each kind of associative activity for each figure.

Results. The results of this treatment of the Rorschach test figures are given in Table I, which gives the sigma values for every one of the figures for every type of associative organization.

TABLE I

SIGMA VALUES FOR EVERY TEST FIGURE FOR EVERY TYPE OF ASSOCIATIVE ORGANIZATION

W = whole response; *A* = combination of distant elements; *B* = combination of white space with solid material; *C* = combination of adjacent material; *F* = analysis-synthesis process.

Type of organization	Rorschach test figure									
	I	II	III	IV	V	VI	VII	VIII	IX	X
W	.46	1.32	2.02	.70	.38	.90	.92	1.63	2.00	2.01
A	2.21	2.09	1.44	1.35	1.96	2.34	1.15	1.15	1.78	1.74
B	1.39	1.69	1.70	1.91	1.59	2.47	1.55	1.60	1.96	2.20
C	1.60	1.16	1.28	1.57	1.06	1.21	.48	1.20	1.20	1.60
F	2.36	2.84	2.37	2.41	1.66	1.69	2.06	2.33	2.26	2.79

Sigma values are thus seen to differ with each figure, for the various associative processes. To the extent that sigma value is an index to difficulty, the difference in difficulty of performing each of the above associated processes is indicated. This difficulty is seen to vary with variation in conditions, as represented by the variation in the 10 figures.

The central tendency and scatter for every type of associative organization is given, for the test series as a whole, in Table II.

[4] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 101-104.

TABLE II

CENTRAL TENDENCY AND SCATTER FOR EVERY TYPE OF ASSOCIATIVE ORGANIZATION

W = whole response; *A* = combination of distant elements; *B* = combination of white space with solid material; *C* = combination of adjacent material; *F* = analysis-synthesis process.

Type of organization	Median	P.E.	$P.E._M$
W	1.120	.6175	.244
A	1.760	.3875	.153
B	1.695	.1825	.072
C	1.205	.1575	.062
F	2.345	.2575	.102

The differences in medians indicate a difference in difficulty as between types of organization, when the test series as a whole is considered. How real this difference is appears in Table III which shows for all the different types the difference in the median sigma values, the P.E. of the differences, and the ratio of the difference to the P.E.

Conclusions that seem justifiable from these findings are: the whole (*W*) response is not more valuable as indicator of organizing activity than responses in which only details are combined. In fact it is less so. But there is no significant difference between the mean sigma value for this response and that for combining adjacent details (*C*). Organization of adjacent details would therefore seem to have the same value as indicator of the associative process as organization of all elements of the material into a whole. The organization of distant details (*A*) and that of white with solid material (*B*) are seen to have significantly higher sigma values than the whole response, with respective chances of 91 and 92 in 100 of a real difference, but not enough for absolutely reliable indication of a real difference. The synthesis-analysis (*F*) process has a sigma value statistically reliably greater than the whole response. Comparing the other

TABLE III

COMPARISON OF THE VARIOUS TYPES OF ASSOCIATIVE ORGANIZATION

Types compared	Diff.*	$P.E._{diff.}$	$D/P.E._{diff.}$
W–*A*	.640 (*A*)	.289	2.22
W–*B*	.575 (*B*)	.255	2.25
W–*C*	.085 (*C*)	.253	.33
W–*F*	1.225 (*F*)	.266	4.61
A–*B*	.065 (*A*)	.169	.38
A–*C*	.555 (*A*)	.165	3.36
A–*F*	.585 (*F*)	.184	3.17
B–*C*	.490 (*B*)	.095	5.14
B–*F*	.650 (*F*)	.125	5.20
C–*F*	1.140 (*F*)	.119	9.58

* The letter in parentheses indicates the process having the higher of the two sigma values.

processes it is seen that organization of distant details is reliably of higher sigma value than organizing adjacent elements, the chances being 99 in 100 that the difference is real. But there is no difference in value between organization of distant details and that of white with solid ones. Since the white details are always adjacent to those with which they are combined, this finding speaks for the difficulty of including a white detail into an organized larger whole. *Gestalt* thinking, with its emphasis on figure, ground, and the contrast of one to the other in the larger percept, would seem to get some support from this finding. This greater difficulty in organizing a white space with an adjacent solid element is clearly brought out in the fully reliable difference between the sigma values for this process, and that of combining adjacent solid material (the *B* and the *C* processes). Finally, it is clear that the analysis-synthesis process is by far the most difficult. Statistically absolute certainty is found that its sigma value is higher than the organization of adjacent details; organization into wholes; organization of white spaces with other details. As compared with organization of distant details, the difference is not absolutely reliable, but the chances are high, almost 98 in 100, of a real difference.

Light on the respective processes is thrown also by the scatter shown in the 10 figures of the test series, for each kind of organization process. The whole response, median 1.12, P.E. .6175 shows a very wide scatter. The ratio of the median to its P.E. is only 1.81. The great dissimilarity of the findings themselves (Table I) reflect the same wide scatter. Production of whole responses is seen thus to be a very variable process. With change of conditions, as represented by the different test figures, the sigma value of the whole response changes. Not so much variation is found in respect to any of the other processes. This appears from the individual sigma values in the respective figures; from the medians; and more especially from the ratio of the medians to their P.E. These values are given in Table IV.

TABLE IV

RATIOS OF THE MEDIANS OF THE DIFFERENT TYPES OF ASSOCIATIVE ORGANIZATION TO THEIR P.E.

Type of organization	$M/P.E._{dis}$
A	4.54
B	9.28
C	7.65
F	9.15

Very narrow scatters are thus indicated, especially as compared with that for whole responses. It seems justifiable to conclude that in so far as the group in the experiment employed the organizing processes under con-

sideration, the varying conditions as represented by the different test figures did not have great effect on the sigma value of the processes. The difficulty of producing processes other than wholes seems, in other words, to remain fairly constant, irrespective of conditions. The findings are graphically portrayed in Fig. 5. The *A, B, C,* and *F* curves follow the form of a straight line much more closely than that for *W*.

Frequent reference has been made in this report to the "conditions" under which the processes were elicited, *i.e.* the figures in the test series forming the stimuli in the experiment. In so far as these can be described

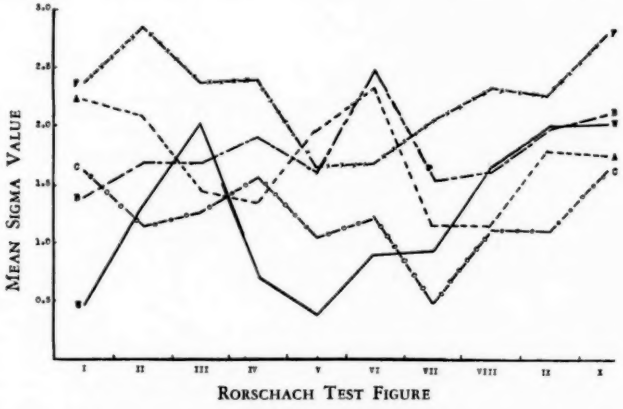


FIG. 5. MEAN SIGMA VALUE FOR EVERY ONE OF THE RORSCHACH TEST FIGURES FOR EVERY TYPE OF ASSOCIATIVE ORGANIZATION

in terms of configuration, shading, color, what information do they give as to the relation between these conditions and the sigma values of the various organizing processes observed?

A precise description of the test figures in terms of every-day meaning is *ex hypothesi* impossible, since the figures start by being meaningless. They are ink-blots, whose form is left to chance, except that all are symmetrical, a characteristic effected by folding in the center the paper on which the figures were originally made.[5] The outer outlines of each half of the test figure are irregular. The amount of white space in each is irregular as compared with the others. The appearance they present to

[5] For a more detailed description of the technique, see Rorschach, *op. cit.*, 4, 10 ff.

the eye, with reference to their solidity, shading, and color is about as follows (Roman numerals refer to the ordinal position of the figure in the test series):

Solidity:	
Definitely solid	I, IV, V, VI.
Discrete elements interconnected	II, VII.
Definitely broken up	III, VIII, IX, X.
Color:	
All black	I, IV, V, VI, VII.
Principally black, with red details, relatively of small amount	II, III.
All colored	VIII, IX.
Principally colored, with some black of relatively very small amount	X.
Shading:	
Shading differential marked	IV, VI.
Shading differential present but slight	I, II, III, V, VII, X.
No obvious shading differential other than color	VIII, IX.
White Space within figure:	
Prominent	I, II, III, VII, VIII, X.
Present in small amount	IV, IX.
None	V, VI.

Considering now the quantitative findings in the light of these descriptions, it seems justifiable to conclude that material is more easily organized into meaningful wholes the less broken up it is. Test figures I, IV, V, solid, have the lowest sigma values in producing whole responses. VII, has almost the same sigma value as VI, which, also solid, is fourth in order. The sixth in order of *W* sigma value, test figure II, is like VII in that it consists of discrete elements interconnected. Thus interconnected discrete material stands next to solid material in respect to ease of organization into a whole. Both of these findings are as would be logically expected. Another constant factor is, however, the presence of color in the 5 test figures having the highest sigma value. There is a perceptible break, too, between VII, fifth in respect to the sigma value of its *W*, 0.92; and figure II, sixth in respect to its *W* sigma value, 1.32. The question presents itself, does presence of color, even when in the relatively small amount found in II and III, hamper organization into wholes?

This is as far as the descriptive aspect of the test figures shows any definite relation to the quantitative findings. In respect to the other processes, no unequivocal relationship is perceptible. In organizing distant details, for example, the highest sigma values are obtained in I, II, and VI, which may be considered as expected: material easily reacted to as wholes

fails to stimulate into seeking non-obvious relationships. But on that reasoning VII and IV should also show high sigma values for this process. They are nevertheless lowest and third lowest respectively. Whether, as above remarked, the amount of *A, B, C,* and *F* process is likely to remain constant, irrespective of conditions, or whether this lack of differentiation is due to uncontrolled conditions in the test series, this writer is not prepared to say.

Uncontrolled conditions do exist, however, and until experimental procedure should show them to be of no or little weight in influencing the findings, it is obviously premature to conclude that irrespective of conditions, a group such as is represented by those participating in the present experiment, will tend to combine distant details in about the same amount, to combine white and filled spaces in about the same amount, combine adjacent details in the same amount, and analyze-synthesize in the same amount.

The uncontrolled elements in this experiment are, principally, color, and ordinal position of the figures. Thus the highest sigma values for whole reactions are found in II, VIII, IX, X, and III. Color composes VIII, IX, and practically all of X, and enters into, although in only a relatively small degree in II and III. Is color an element interfering with organization into a whole? Is the position of VIII, IX, and X, towards the end of the series, a factor? Shading and amount of white space within the figure are also uncontrolled in this experiment. To what extent does contour, within a figure, produced by differential shading, promote response to details and to that extent militate against organization into wholes? If white space is important in breaking up a figure and so making difficult the organization into wholes, why the high sigma value for *W* in test figure IX, with its relatively small white space? Again, is it because of the colors, facilitating separation and militating against organized interpretation? or is it ordinal position, with possible fatigue of the subject?

These are among the outstanding questions that present themselves, and that cannot be answered until checked by control experiment.

Summary

The Rorschach test, an ink-blot series, was administered to 39 individuals of very superior intelligence. Their responses were studied with reference to the evidence of organization of details of the test figures into larger meaningful combinations.

(1) The principal types of organization found were of (a) wholes, (b) distant details, (c) white spaces with solid material, (d) adjacent details, and (e) analysis-synthesis.

(2) Sigma values for each organization process in each figure of the test series were obtained. These were found to vary for each process in each figure. The analysis-synthesis process was found having a sigma value reliably higher than all the other processes except the organization of distant details; and significantly higher than organization of distant details. A significant and practically reliable difference in favor of the organization of distant details is found as against combination of adjacent details; a significant difference is also found in the sigma value of the organization of distant details as against wholes. The organization of white spaces with adjacent solid ones also has a sigma value significantly higher than combination of adjacent solid material, and organization into wholes. The median sigma value of the wholes does not significantly differ from that for organization of adjacent solid details into meaningful relationship. The sigma values for wholes in the respective figures varies sufficiently to assign different values for whole responses in each figure, when the test is used diagnostically. Organization into wholes was found definitely to vary with change in the conditions as represented by the figures in the test series. The other organization processes did not show so much variation. Their sigma values remain fairly constant for all figures in the series. Unchecked variables in the experiment are, principally, color and ordinal position of the test cards.

PSYCHOLOGICAL SIGNIFICANCE OF CEREBRAL BIRTH LESIONS

By EDGAR A. DOLL, The Training School at Vineland

Human birth is attended by many dangers, one of which is the possibility that serious damage may be done to the intracranial portion of the central nervous system. This damage may be produced during the period of labor or it may be the result of abnormalities occurring during the later stages of pregnancy. Since the effects of very early postnatal cerebral pathology are much the same as those incurred prenatally or during labor, it is customary to consider intracranial birth lesions as including all those which develop within approximately one or two months before or after birth, as well as those occurring during delivery.

It is impracticable to discuss here the pathology or the etiology of cerebral birth lesions. In this paper we are more immediately concerned with the significance of such lesions in relation to mental development.[1] It is impracticable even to discuss the details of these effects, except to note that birth lesions may produce (1) motor impairment, (2) retardation of intelligence, (3) disturbance of personality and conduct, with (4) consequent handicaps in learning. These various aspects of behavior may be involved singly or in combination, and the effects may be mild or severe. Impairment in central sensory mechanisms is a presumptive further consequence, but it has received comparatively less attention.

The motor difficulties which follow from cerebral birth lesions constitute the most conspicuous feature of the condition. These signs also provide the principal basis for clinical diagnosis. Among these, spasticity and athetosis are the most important.

For practical purposes, spasticity is defined as essentially simultaneous contraction of reciprocal muscle groups, with hypertonicity. Athetosis may be described as successive involuntary movements superimposed upon fairly good voluntary coördination. These two types of motor difficulty are seldom seen alone, and are often combined with overflow (synkinesia), ataxia (incoördination), or tremors.[2] The motor difficulties may be due to dis-

* Accepted for publication March 20, 1933. From the Research Department of the Training School at Vineland, New Jersey. The author wishes to acknowledge his indebtedness to Dr. Winthrop M. Phelps, of Yale University, for medical collaboration in the case-studies on which this report is based, and for both general and specific suggestions in the interpretation of the data.

[1] For discussions of pathology and etiology see E. A. Doll, W. M. Phelps, and R. T. Melcher, *Mental Deficiency Due to Birth Injuries*, 1932; and H. Ehrenfest, *Birth Injuries of the Child*, 2nd ed., 1931.

[2] W. M. Phelps, Cerebral birth injuries, their orthopedic classification and subsequent treatment, *J. Bone & Joint Surg.*, 14, 1932, 773-782.

turbances in tone, timing, reciprocation, power, or steadiness, and the involvement may affect the trunk or extremities separately or in combination.[3] Speech may or may not be involved.

These motor disturbances have important bearing on the neuromuscular background of behavioral development. Both maturation and learning are involved, because the lesion presumably interferes with the growth of neurological elements and with the development of expressive behavior. It is evident that the motor handicaps tend to lessen with age. This suggests delayed growth, repair, or substitution of central neural elements. Improvement takes place also under physical therapy, and the mechanism of such improvement is an important problem of learning.

It is evident that the site of the lesion or its extent may have an important bearing on the temporal and directional phases of neuromuscular development. In our subjects the characteristic developmental motor sequences of normal infancy and childhood are followed as an average tendency, although these developments are grossly delayed.

The development of speech in these subjects is of special importance, not only with reference to cerebral localization but also with respect to the motor basis of ideational development. Neuromuscular defects of speech are present in practically all of our mentally subnormal subjects, but among the mentally normal birth-injured, speech defects are apparently less frequent. However, serious impairment of speech, as such, seems to have relatively little influence, among the birth-injured, on the development of intelligence as expressed in comprehension and ideational response. Our subjects as a group show no significant correlation between speech-impairment and intellectual impairment.

The question arises whether retardation in motor development has a permanent effect upon the degree or rate of such development when it does take place. Whether there are critical genetic periods for the expressive development of particular motor or intellectual functions is of some interest here. If so, does the inhibition of such expression at the normal period of maturation interfere with the rate or degree of subsequent acquisition of such functions? From our results, we cannot offer a definite answer, but apparently the ultimate level of behavioral development cannot safely be predicted from a given point of temporal retardation.

Impairment of intelligence is one of the important consequences of cerebral birth lesion.[4] This is not, however, a necessary consequence, since many subjects show no impairment of intelligence except indirectly in the ex-

[3] *Idem.*, Dyskinesia, *Yale J. Biol. & Med.*, 3, 1930, 39-48.
[4] Doll, Phelps, and Melcher, *op. cit.*

pressive handicaps of speech and movement. The level of intelligence ranges all the way from absolute idiocy to superior adult levels.

From 6 to 10% of all of our mentally deficient subjects at Vineland have motor handicaps due to cerebral birth lesions. The percentage of persons with birth lesions in the mentally normal population is not known, but is probably not more than 1/10%.[5] Assuming that as many as 2/10% of the general population are birth-injured (which is probably a high figure), and assuming that only 6% of the feeble-minded are birth-injured (which is probably a low figure), then birth injury would be 30 times as frequent among the feeble-minded, in general, as among the non-feeble-minded, in general.

It is generally assumed that about 2% of all children of school age are mentally deficient. Apparently at least 40% of all birth-injured children of school age are mentally deficient.[6] From which it would seem that mental subnormality is perhaps 20 times as frequent among birth-injured children as among physically normal children.

As noted above, intelligence among the birth-injured ranges from complete idiocy to superior adult levels. What is not so clear is the relation of the site and extent of cerebral lesions to intellectual development. This problem is complicated by the relation of speech to intelligence and the uncertainties of cerebral localization with respect to speech, and it is further complicated by the relation of handedness to speech and to intelligence, in terms of cerebral localization. One can only surmise to what extent a cortical hemorrhage might spread along the brain surface or within its depths and across its lobes. The final answer to these questions will probably be learned from post-mortem examinations.

In the meantime, it is apparent that there is no significant correlation between the level of intelligence and the severity of the motor impairment. Preliminary results suggest, however, that our spastic subjects show somewhat more mental impairment than do our athetoid subjects, and this result may possibly be related to the cortical damage among spastics as contrasted with the presumptive damage in the basal nuclei among athetoids.

The problem of intelligence in these cases is further complicated by the difficulty of measuring intelligence in the presence of expressive handicaps of speech and movement. Skill, patience, tact and experience are necessary

[5] This estimate is based on general reports that seven children per thousand are cripples, supplemented by the fact that at the Spalding School (Chicago) one cripple in seven is a congenital paralytic. The estimate is only a tentative working figure.

[6] One-third of the congenital paralytics at the Spalding School are reported mentally subnormal. G. B. Smith reports three-fourths of infantile cerebral accidents mentally subnormal.

if one is to avoid underestimating or overestimating the intellectual level among subjects with motor handicaps. Where the motor impairment does not involve speech, or is confined to movements which do not involve manipulative performance, there is, of course, no serious difficulty of mental measurement. In many cases one is immediately impressed with the obvious normality of intelligence in subjects where speech and movement are almost totally absent, although this impression cannot easily be sustained by current methods of objective examination.

The relation of handedness to intelligence becomes, under these circumstances, a matter of special importance, but one of more difficulty than at first appears. For example, in hemiplegia it sometimes appears that the affected side is, nevertheless, the preferred side in manipulative performance. But if the opposite side is preferred, one cannot be sure whether this is a native preference, possibly inherited, or one acquired through necessity.

Left-handedness seems to be somewhat more common among the feeble-minded than among the mentally normal, but this is not obvious from casual observation. At any rate, the number is certainly not so great as to favor the thesis, advanced by some brain surgeons, that the neurological seat of the intelligence is in the left frontal lobe, or even in the left hemisphere. On the other hand, it seems quite apparent that left-handedness is very much more common among our birth-injured mentally deficient subjects than among our feeble-minded group in general, but this fact has not yet been correlated with the localization of the motor damage. It is clear that further study of these subjects should shed important light on the thesis of unilateral localization of intelligence. This information can then be correlated with that gained from cerebral hemisection and other surgical procedures in cases of brain pathology.

The development of speech in these cases is another field of special interest because of the relation of language to thought and the impairment in language development in subjects with grave motor handicaps of speech. We have seen obviously normal intelligence in young children with almost total speech handicap, and obviously superior intelligence in adults who have never developed intelligible speech.

Mental growth is of particular interest in these subjects, because of its unusual character, being seriously delayed or irregular in many cases, but apparently unimpaired in others. A study of our group as a whole shows a decided tendency for the intelligence to improve with age, not only absolutely, but also relatively. Increasing development in the later years and continuing beyond the usual age-limits is particularly noteworthy.

Thus all but one of twelve cases studied for fairly long periods of time showed unusual mental growth not characteristic of the ordinary feeble-minded.[7] Individual cases are especially impressive. Thus, one child with a Binet mental age of 4 at life-age 8 (I.Q. 50), gained four years in mental age during the next four years, a rate of growth which was twice the I.Q. prediction. Another child, with a Binet mental age of 6 at life-age 12 (I.Q. 50), made four years' mental growth in the next four years. A third case, with I.Q. 100 at life-age 5, made no appreciable mental development during the next three years, but during the following five years increased five years in mental age.

These particular growth records are independent of the difficulties of mental examination and there is no evidence that anything unusual occurred in the treatment or training. Instances of remarkable functional improvement late in life apparently capitalizing a continuing maturation of intelligence not previously expressed in behavior are not unusual. The remarkable extent to which intelligence, if present, can surmount the motor handicaps of speech and movement is another of the notable features in these subjects.

As previously noted, disturbances of personality and disorders of conduct are among the consequences of birth lesions.[8] It is not clear whether these disturbances are the direct consequences of structural or physiological involvements, or whether they are the indirect result of the frustration in behavior imposed by the motor handicaps. It appears, however, that personality or disposition may be affected independently of intelligence and movement.

In view of the uncertainties of the constitutional bases of conduct and personality, this problem is even more difficult of interpretation than that of intelligence. Some authorities suggest cortical localization of personality and this suggestion may ultimately receive some support from the study of the birth-injured.[9]

In our subjects, it appears that the sympathetic nervous system is often involved, with consequent visceral disturbances which could produce grave disturbances of mood or behavior. According to present evidence, it seems unlikely that the glands of internal secretion are disturbed by cerebral birth lesions, although this is a problem which merits further attention. It has

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[7] Doll, Phelps, and Melcher, *op. cit.*

[8] P. L. Schroeder, Behavior difficulties in children associated with the results of birth trauma, *J. Amer. Med. Assn.*, 92, 1929, 100-104.

[9] R. J. A. Berry, Cerebral cortical structure and its relation to mental disease, *Brit. Med. J.*, 1, 1931, 837-839.

been known for some time that visceral disturbances can produce focal infections which affect cerebral function.

Our own observations in the direction of personality and conduct are too limited to warrant much emphasis. At the present time, these observations are clinical rather than statistical. They support the general assumption of dispositional involvement. These involvements are readily observed in the mentally low-grade patients, where excitability, irritability, distractibility, hyperactivity, apprehensiveness, and the like, are common traits. In our mentally high-grade cases we are more impressed with the peculiarities of attitude as reflected in poor insight, poor social judgment, obtrusiveness, and egocentricity. Favorable involvement of personality is reflected in a conspicuous persistence and concentration of effort present in some cases, even in the face of distractibility and irritation provoked by continuous frustration of voluntary effort.

Summary of Data

Thirty-nine cases of mental deficiency, about 9% in an institutional population of 440, have been classified as associated with birth lesions. Ten of these are without gross motor signs; the remainder show various types and degrees of paralysis. They have been grouped as *definite* (12), *probable* (15), and *possible* cases (12). This classification is based on the relative importance of various aspects of the natal and developmental histories.[10] There is a strong presumption that in both the motor and non-motor groups the birth lesion is a probable cause of the mental deficiency as well as of the motor handicaps.

Motor group. In the group with motor signs, 83% are found to have positive birth histories, 65% have positive neonatal histories, 83% have positive genetic motor developmental histories, 83% have negative other pathology, 90% have negative heredity (for both motor and mental status), and 100% have positive symptoms for present status.

As to the type of involvement in the motor group, 14 subjects are classed as spastic, 9 as athetoid, 1 as mixed, 2 as intention-tremor, and 3 as undifferentiated. Of the spastics, 4 are paraplegias, 6 are quadriplegias, 1 is a right hemiplegia, 2 are left hemiplegias, and 1 is a triplegia.

Broadly speaking, the spasticity is of cortical pyramidal origin, while the athetosis is of basal ganglion origin.

Of the subjects with motor signs, 83% are males. The median life age is 16 years, the median mental age 6.8 years, and the median I.Q. 55. These distributions correspond very closely to the institutional group as a whole.

Of the subjects with motor signs, 66% are first-born, 24% are premature births, and 41% are left-handed. In the total institutional group, 22% are first-born, no instances of prematurity are found in a control sample, and 12% of a control sample are left-handed.

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[10] E. A. Doll, Birth lesion as a category of mental deficiency, *Amer. J. Orthopsychiat.*, 3, 1933, 1-13.

STATISTICAL DATA ON BIRTH-INJURED, MENTALLY DEFICIENT SUBJECTS

	No. cases	%	Med. L.A.	Med. M.A.	Med. I.Q.	(%) Male	(%) First-born	(%) Prema-ture	(%) Left-handed
Spastics	14	36	19	7.0	51	93	72	21	36
Athetoids	9	23	15	6.7	61	56	44	33	44
Other motor	6	15	15	6.5	60	100	83	17	50
Total motor	29	74	16	6.8	55	83	66	24	41
Non-motor	10	26	16	4.0	29	90	80	30	10
All B-I cases	39	9	16	6.7	51	85	69	26	33
Institution	440	—	18	6.9	54	62	22*	0*	12*

*Sampling

	No. cases	Positive Clinical Status	Positive Birth History	Positive Neonatal Condition	Positive Motor History	Negative Other Pathology	Neg. Heredity
				(Percentages)			
Motor cases							
Definite	12	100	92	83	92	100	91
Probable	13	100	77	54	77	77	85
Possible	4	100	75	50	75	50	100
Total motor	29	100	83	65	83	83	90
Non-motor	10	100	90	90	90	80	100
All B-I cases	39	100	85	72	85	82	92

	Correlations R	r	P.E.$_r$	
Mental age—life age	.34	.51	.09	
Mental age—motor facility	−.05	−.07	.13	
(same, corrected for life age)		.06		.13
Mental age—speech facility	−.01	−.01	.13	
(same, corrected for life age)		.10		.12
Mental age—genetic score	.53	.74	.06	
(same, corrected for life age)		.67		.07
Life age—motor facility	−.16	−.25	.12	
(same, corrected for mental age)		−.24		.12
Life age—speech facility	−.11	−.17	.12	
(same, corrected for mental age)		−.20		.12
Life age—genetic score	.32	.48	.10	
(same, corrected for mental age)		.17		.12
Motor facility—speech facility	.20	.31	.11	
(same, corrected for M.A. and L.A.)		.27		.12
Motor facility—genetic score	.07	.11	.12	
(same, corrected for M.A. and L.A.)		.29		.12
Locomotion—manipulation	.38	.56	.09	
(same, less 4 paraplegias)	.86	.98		.01

Non-motor group. Ten subjects have been classified in the birth-lesion group without gross motor signs. Nine of these have positive birth histories, positive neonatal histories, and positive genetic motor retardation. Eight have negative other pathology. All have negative heredity. Two are classed as probable, and 8 as possible birth lesions. One is female and 9 are males. The median I.Q. is 29, (life

age 16, mental age 4.0). Eight are first-born; 3 are premature; and 1 is left-handed.

Spasticity vs. Athetosis. Nine athetoids in the group of 29 cases with motor signs have a median I.Q. of 61 (life age 15, mental age 6.7). Fourteen spastics in this group have a median I.Q. of 51 (life age 19, mental age 7.0), a fact which may bear some relation to the presumptive cortical basis of spasticity as contrasted with the basal-ganglion defect in athetosis. Of the athetoids, 44% are left-handed, and of the spastics, 36%. Since athetosis represents principally an involvement of the basal ganglia, this fact suggests that handedness may be related to subcortical as well as to cortical localization.

Three of the 14 spastics (20%) are hemiplegias. Two of these are left hemiplegias, and 1 is right. (Sachs and Peterson, in a group of 156 hemiplegias, found 52% right-sided, and 48% left-sided.)[11] The I.Q. of the right-sided hemiplegia is 49, the average I.Q. of the two left-sided hemiplegias is 62. Among 6 spastic quadriplegias, 3 are left-handed, 2 are right-handed, and 1 is ambidextrous.

Handedness. Twelve of the 29 motor cases (41%) are left-handed, 15 cases (50%) are right-handed, and the remainder ambidextrous. (This may be compared with the figures for hemiplegias of Sachs and Peterson—52% and 48%—suggesting that birth lesions are not unilaterally selective.) The median I.Q. of the left-handed group is 50 (life age 15, mental age 5.6), while for the right-handed group it is 53 (life age 17, mental age 7.4). This slight difference is due to a slightly lower percentage of spastics in the left-handed group as compared with the right-handed group (42% and 53%), the athetoids being equal numerically in the two groups.

In a control group of 33 subjects, alphabetically selected, 4 subjects (12%) were left-handed, 26 subjects (80%) right-handed, and the remainder ambidextrous. The median I.Q. of the left-handed was 60, and of the right-handed 59. (Thus left-handedness is four times as frequent in the birth-lesion group as in the institutional group, and is about twice as great for the latter as compared with the general population.) In the non-motor group, one of the 10 cases is reported left-handed.

Correlations. In order to obtain an idea of the trend of relationship of the motor handicaps of these subjects to age and mental age, the 29 subjects with motor signs were ranked according to facility in locomotion, manipulation and speech, and these were correlated with life age, mental age, and with each other. These ranks were based on the composite judgment of several observers. The correlations are not very dependable because of the subjective nature of the ranks and the small number of cases. The coefficients were obtained by the Pearson footrule formula $R = 6\Sigma g/n^2$, and these values were converted into the Pearson r. The latter coefficients were then corrected for related variables.

The correlation between mental age and life age is $r = 0.51$, P.E. 0.09. The correlation between mental age and motor facility (locomotion and manipulation combined), corrected for life age, is $r = 0.06$, P.E. 0.13. Between mental age and speech facility, corrected for life age, the coefficient is $r = 0.10$, P.E. 0.12. The correlation between life age and motor facility, corrected for mental age, is $r = -0.24$, P.E. 0.12. Between life age and speech facility, corrected for mental age, the

[11] B. Sachs and F. Peterson, A study of cerebral palsies of early life based upon an analysis of 140 cases, *J. Neur. & Ment. Dis.*, 17, 1890, 295-332.

coefficient is $r = -0.20$, P.E. 0.12. The correlation between motor facility and speech facility, corrected for both mental age and life age, is $r = 0.27$, P.E. 0.12. The correlation between facility of locomotion and facility of manipulation is $r = 0.56$; this coefficient is increased to $r = 0.98$ when four paraplegias without handicap of manipulation are eliminated.

The correlation between facility in locomotion and manipulation is positive, high, and reliable, revealing a marked tendency toward generalized motor handicap except in the case of paraplegias. The coefficients of correlation between motor facility and speech facility and each of these with life age and mental age tend toward an absence of correlation. From clinical observation, one would expect motor handicap and speech handicap to be positively correlated with each other and negatively correlated with both age and mental age in a fairly marked degree.

Genetic development. An attempt was made to study the genetic behavioral development of the 29 subjects with motor lesions in terms of general functional ability. The behavioral status of these 29 subjects, as obtained from a tentative scale of functional development, shows about a 4-year level of performance. This is about 3 years below the median Binet mental age of the group. The correlation between this genetic score and Binet mental age, corrected for life age, is $r = 0.67$, P.E. 0.07. The correlation between life age and genetic score, corrected for mental age, is $r = 0.17$, P.E. 0.12. The correlation between genetic score and motor facility, corrected for both mental age and life age, is $r = 0.29$, P.E. 0.12. These coefficients show a definite tendency for the genetic score to be more dependent upon mental age than upon either life age or motor facility.

The psychological study of children with birth lesions should be especially important in relation to current theories of learning. The neuromuscular handicaps in the motor cases, together with the involvement of intelligence and personality in other cases, afford an unusually fertile field for special study. The organization of behavior in these children, following as it does the general motor sequences of normal infancy, but at very much retarded periods or rates, is of value, not only for the slow-motion study of behavioral development, but also for its neurological relations. The neuromuscular theory of learning, the maturation of learning, the linguistic correlates of learning, the control of voluntary and involuntary movements through growth and muscle-training, are problems which are immediately brought into relief in the study of the birth-injured.

SPATIAL PERCEPTION IN THE REGION OF THE OPTIC DISK

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The blind spot has, since its discovery by Mariotte in 1668,[1] been the subject of many investigations and the focal point of numerous controversies.[2] Many of the questions thus raised during the past 265 years are still undecided, not because they have been neglected, but because, on the contrary, so many different answers have been returned that generalization is difficult if not impossible. One of these questions—the specific problem of this study—concerns the spatial values of the retina in the region of the optic disk. Numerous studies have been made of this problem, and we venture to add one more to the list because the results and statements of the different investigators have been widely at variance.

Logically, there seem to be three possible answers to our question: the spatial values of the retina in the region of the optic disk might (*1*) be distorted by way of contraction, (*2*) be distorted by way of expansion, or (*3*) be unchanged except for the distortion characteristic of peripheral vision. Not only have all these possibilities received experimental validation, but, strangely enough, this gamut has been exceeded. Some investigators have found (*4*) that the spatial values are equivocal, decreasing or increasing as conditions are variously altered. The experiments leading to these very different conclusions may be grouped as follows.

(1) Contraction or shrinkage. The question regarding the spatial values of the retina in the region of the optic disk was first raised by Volkmann in 1846.[3] To demonstrate his theory of local signs, Volkmann cited the fact that a figure subtending the blind spot appeared very much shortened. Though he ignored, or at least overlooked, this observation when he came, seven years later, to different conclusions respecting the spatial values of the region of the optic disk, other experimenters (Von Wittich,[4] Funke,[5] Bull,[6] Brückner,[7] Ferree and Rand,[8] Werner,[9]

* Accepted for publication May 30, 1931. From the Psychological Laboratory of Columbia University.

[1] E. Mariotte, *Mém. de l'acad. de Paris, trans. phil.*, 3, 1668, 668.

[2] It has also beeen a continual source of surprise and amusement to the laity. The "merry monarch" Charles II of England (1630-1685) is said to have used it in jest to show his courtiers "how they would look when their heads were off their shoulders" (Helmholtz, *Physiological Optics*, 2, 1925, 29), and students and others today evince amusement as well as surprise when it is first brought to their attention.

[3] A. W. Volkmann, Sehen, in Wagner's *Handwörterbuch der Physiologie*, 3, 1846, 265-351.

[4] W. H. von Wittich, Studien über den blinden Fleck, *Arch f. Ophth.*, 9, 1863, 1-31.

Lohmann,[10] Hofmann,[11] and Helson[12]) have brought forward evidence of contraction or shrinkage.

(2) Expansion or magnification. The phenomenon of expansion has been reported in only one study, that by Ferree and Rand. In addition to noting the fact that "complete shrinkage takes place when the stimuli observed are on the edge of the blind spot,"[13] they also observed that "there is a rapid falling off of the influence of the blind spot upon spatial values as the stimuli recede from its edges,"[14] until finally a point is reached where there is no distortion. This phenomenon, they explain, is due to the fact that "the shrinkage in the visual field produced by the drawing together of [the edges of the blind spot] is compensated for by a magnification of spatial values in the region immediately surrounding."[15]

(3) No change. Despite Volkmann's priority, as documented above,[16] Weber is usually credited with having made, in 1852, the first observations on the spatial values of the region of the optic disk.[17] He found no changes other than those characteristic of peripheral vision in general. The following year (1853) he,[18] Volkmann,[19] and Fick and DuBois-Reymond,[20] working independently, corroborated this finding, and since then—despite reports by careful workers to the contrary—the weight of opinion and the preponderance of experimental evidence has been against deformation. In addition to the studies reported in 1853, the following have confirmed Weber: Aubert,[21] Woinow,[22] Wundt,[23] Helmholtz,[24] Titchener,[25] Landolt,[26] Brückner,[27] Nussbaum,[28] Feinberg,[29] Bard,[30] and Hartmann.[31]

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[5] O. Funke, Zur Lehre vom blinden Fleck, *Ber. d. naturf. Gesell, z. Freiburg i. Br.*, 3, 1864, 12 f.

[6] O. Bull. *Perimetrie,* 1895, 8.

[7] A. Brückner, Über die Sichtbarkeit des blinden Fleckes, *Arch. f. d. ges. Physiol.*, 136, 1910, 610-657.

[8] C. E. Ferree and G. Rand, The spatial values of the visual field immediately surrounding the blind spot and the question of the associative filling in of the blind spot, *Amer. J. Physiol.*, 29, 1912, 403 f.

[9] H. Werner, Untersuchungen über den blinden Fleck, *Arch. f. d. ges. Physiol.*, 153, 1913, 475-490.

[10] W. Lohmann, Der blinde Fleck in seinen Beziehungen zu den Raumwerten der Netzhaut, *Arch f. Augenhk.*, 81, 1916, 181-196.

[11] F. B. Hofmann, *Die Lehre vom Raumsinn des Auges,* 1, 1920, 190-198.

[12] H. Helson, the effects of direct stimulation of the blind-spot, this JOURNAL, 41, 1929, 345-397. An excellent summary of the literature on the blind spot is given in this study.

[13] Ferree and Rand, *op. cit.*, 403.

[14] *Ibid.*, 405.

[15] *Ibid.*, 406.

[16] Volkmann, *op. cit.*

[17] E. H. Weber, Über den Raumsinn und die Empfindungskreise in der Haut und im Auge, *Ber. d. k. Sächs Ges. d. Wiss.*, 4, 1852, 185.

[18] Weber, Über Grösse, Lage und Gestalt des sogenannten blinden Flecks im Auge und die davon abhängigen Erscheinungen, *ibid.*, 5, 1853, 149-158.

[19] A. W. Volkmann, Über einige Gesichtsphänomene, welche mit dem Vorhandensein eines unempfindlichen Fleckes im Auge zusammenhängen, *ibid.*, 5, 1853, 27-50.

[20] A. Fick and P. DuBois-Reymond, Über die unempfindliche Stelle der Netzhaut im menschlichen Auge, *Arch. f. Anat., Physiol., u. wiss. Med.*, 1853, 396-407.

[21] H. Aubert, Über den blinden Fleck, *Jahresber. d. Schles. Gesell.*, 1854, 25-28.

[22] A. Woinow, Über das Sehen mit dem blinden Fleck und seiner Umgebung, *Arch. f. Ophth.*, 15, 1869, (pt. 2), 155-166.

(4) Equivocal. Some investigators, again, hold the opinion that the spatial values in the region of the optic disk are equivocal. This view was early advanced by Funke. He found, in 1864, that these spatial values varied sometimes in one direction and sometimes in another, depending upon the conditions of the experiment.[32] In the following year, 1865, Aubert wrote, "although I have had great experience in indirect division, . . . I am bound to state publicly that I am not able to reach any decision as to how the field of view is completed at this point" [*i.e.* at the blind spot].[33]

The claim of equivocality was not advanced again until 1912, when Ferree and Rand reported an experiment showing that the spatial values varied specifically with the areas stimulated.[34] Some areas showed 'shrinkage,' some 'expansion,' and some no change at all. Though their results would seem to reconcile the discrepancies of the earlier studies, they have never been confirmed. They not only stand alone, but they have been met with the charge of being artifacts. Nussbaum claims, and his criticisms have never been refuted, that Ferree and Rand's results are due to faulty fixation and involuntary movements of the eyes.[35]

PROBLEM

This diversity of the results is a challenge for further work. The problem is specific. The different findings of the earlier investigators cannot, however, be resolved through a repetition of the usual procedures. All that the old procedures can possibly yield are results aligning the experimenter with one or another of the groups mentioned above. A new approach must be made. Many experimenters have compared spatial perception in the region of the optic disk with perception in neighboring or symmetrically

[23] W. Wundt, *Grundzüge der physiologischen Psychologie,* 2, 1893, 104; 2, 1902, 510.

[24] H. von Helmholtz, *Handbuch der physiologischen Optik,* 1896, 717-727. Eng. trans. 3rd ed., 3, 1925, 210.

[25] E. B. Titchener, *Experimental Psychology,* 1 (pt. ii), 1901, 28 f. Titchener writes, "Although . . . shrinking has in fact been reported by some observers, it seems to be due rather to a 'suggestion' from the blind spot itself than to actual observation. . . . The blind spot, then, has the same spatial value as the surrounding portion of the retina."

[26] E. Landolt, Beobachtungen über die Wahrnehmbarkeit des blinden Fleckes, *Arch. f. Augenhk.,* 55, 1906, 108-118.

[27] A Brückner, Über die Sichtbarkeit des blinden Fleckes, *Arch. f. d. ges. Physiol.,* 136, 1913, 610-657.

[28] F. Nussbaum, Über die Raumwerte in der Umgebung des blinden Flecks, *Arch. f. Augenhk.,* 87, 1921, 142-151.

[29] N. Feinberg, Experimentelle Untersuchungen über die Wahrnehmung im Gebiet des blinden Flecks, *Psychol. Forsch.,* 7, 1926, 22 ff.

[30] L. Bard, Le problème de la tache aveugle et son importance en physiologie générale, *Arch. d'ophth.,* 44, 1927, 5-13.

[31] E. Hartmann, Le problème de l'effacement de la tache aveugle dans le champ visuel, *Ann. d'ocul.,* 164, 1927, 329-336.

[32] Funke, *op. cit.*

[33] H. Aubert, *Physiologie der Netzhaut,* 1865, 257 f. Quoted from the English translation of Helmholtz's *Physiological Optics,* 3, 1925, 207.

[34] *Op. cit.*

[35] Nussbaum, *op. cit.*

located retinal areas, but no one has, as far as we have been able to discover, checked these comparisons with a control experiment upon two retinal areas *both outside the blind spot.* Until such a check is made, no final conclusion can be drawn regarding spatial perception in the region of the optic disk. We report in this study the results of this new attack upon the problem.

Method and Procedure

Two experiments were made. In the first, the Main Experiment, we elicited comparisons of the size of two objects, one falling in the region of the optic disk and the other in a symmetrically located area on the opposite side of the fixation-point (A and B in Fig. 1). In the second, the Control Experiment, both objects were placed symmetrically *above* and on opposite sides of the fixation-point (C and D in Fig. 1).

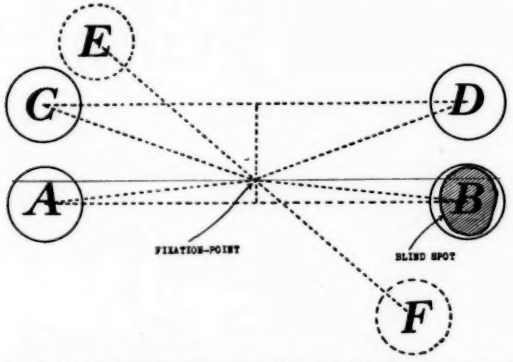

Fig. 1. Diagram Showing the Spatial Relations of the Objects Compared

Disks were suspended at A and B in the Main Experiment, and at C and D in the Control

The distance of the objects from the fixation-point was varied slightly for the different *O*s according to the distance of *O*'s blind spot from the fixation-point. The 'blind spot distance' was used in the Main Experiment, but in the Control Experiment the distance was slightly greater, being the hypotenuse of a right triangle one side of which equaled the 'blind spot distance,' and the other of which was sufficiently large to keep object D (see Fig. 1) outside of the blind spot. The inter-object distance, however, was the same in both experiments. Both distances could not be kept constant without introducing a radical change. If the fixation-distance was held constant, the inter-object distance would be less; and if the inter-object distance was held constant, the fixation-distance would be greater. We could, of

course, have kept both of these distances constant by placing the objects, in the Control Experiment, *on a diagonal,* as for example at E and F in Fig. 1; but we hesitated to introduce such a radical difference in the conditions of the experiment as a new orientation of the objects might involve. We decided, then, upon the slight lengthening of the fixation-distance as the least disturbing change.

The objects were presented on a large white campimeter, which extended nearly to the limits of monocular vision. The campimeter was illuminated by two 400-watt daylight globes. *O* was seated 110 cm. from the fixation-point (a ½-cm. black

TABLE I

SHOWING, FOR EVERY *O*, SIZE OF STANDARD AND VARIABLE AND PERCENTAGE OF REPORTS 'LARGER' IN EACH SPACE ORDER IN BOTH EXPERIMENTS

	Diameter of Standard (in cm.)	Diameter of Variable (in cm.)	Reports			
			Main Experiment		Control Experiment	
O			1st Order	2nd Order	1st Order	2nd Order
		22	0	2	0	0
		23	9	29	13	16
C	24	24	57	54	52	53
		25	86	78	92	92
		26	100	89	99	100
		23	0	2	4	14
		24	32	14	24	44
D	25	25	62	50	54	54
		26	94	76	80	86
		27	100	88	84	98
		21	0	6	12	18
		22	24	56	36	44
H	23	23	58	98	82	84
		24	90	100	94	96
		25	100	100	100	100
		23	16	14	26	0
		24	34	38	34	18
J	25	25	70	52	72	56
		26	94	80	88	92
		27	100	100	98	94

dot), his head held firmly by means of a biting board[26] so adjusted that his right eye (his left being blindfolded) was directly in line with the fixation-point.

The blind spot of the right eye of every *O* was plotted centrifugally along 24 radii. Then a standard and 5 variable objects—black disks—were selected for every *O*. The radius of the standard disk was in every case ½-cm. larger than the longest radius of the *O*'s blind spot. The diameters were 24, 25, 23, and 25 cm. for *C, D, H,* and *J* respectively, The diameters of the 5 variables differed by steps of 1 cm., the median equaling the standard (see Table I). The method of constant stimulus differences was used.

Observers. The *O*s were 4 in number: Mr. R. Hamlin (*H*), graduate student in

[26] The biting board was a small piece of balsa wood, ¼ in. thick. This wood is soft and readily takes and holds the impressions of *O*'s teeth. Such a biting board is as serviceable as one of wax, and is much more easily made.

psychology; Mr. W. B. Curtis (*C*) and Mr. H. E. Johnson (*J*), students in medicine; and the senior author (*D*).

Procedure. Preliminary experiments acquainted the *O*s with procedure and task. A standard disk was hung in place, and then a variable, screened from *O* by *E*'s body. At the signal "Ready," *O* tightened his fixation, and at "Now," *E* stepped aside toward the periphery of *O*'s visual field, and thus exposed the second disk.

O was directed to keep his open eye upon the fixation-point and to compare the size of the second disk (the one exposed by *E* when he moved out of the visual field) with that of the first in the terms 'greater,' 'equal,' or 'smaller.' A few reports of 'doubtful' were returned by every *O*, but these comparisons were later repeated without *O*'s knowledge.

Two series, one for each space order, were conducted in both the Main and the Control Experiments. The standard was at the right in the first space order, and at the left in the second. Every *O* gave 100 reports for every one of the 5 variables in each of the two space orders.

Results

The gross results of the study are presented in Table I, which shows the percentage of reports classed as 'larger,' distributed according to experiment, time-order, variable, and *O*. The percentages given in this table include all the reports of 'larger' and one-half of the reports of 'equal.'

The 'limens' or constants, representing the point of subjective equality,[37] are given in Tables II and III and in Fig. 2. Table II gives these values and their measures of precision (h), for every *O* for each space order in both experiments. The third column gives the diameters of the *O*'s standards, and

TABLE II

Showing for Every *O*, Limens and Measures of Precision for Each Space Order in Both Experiments

(Values in cm.)

O	Experiment	Standard	1st space order (Standard at right)		2nd space order (Standard at left)	
			L	h	L	h
C	Main	24	23.9895	.8436	23.9960	.4893
	Control	24	23.9319	.8614	23.8899	.8901
D	Main	25	24.5526	.6835	25.2279	.5577
	Control	25	25.0661	.4693	24.4965	.4915
H	Main	23	22.7458	.6972	22.1202	.7596
	Control	23	22.2529	.6872	22.0501	.6559
J	Main	25	24.3369	.5955	24.7045	.4262
	Control	25	24.4581	.3917	24.8800	.6360

[37] For other methods of obtaining the point of subjective equality see E. G. Boring, Urban's tables and the method of constant stimuli, this Journal, 28, 1917, 288 f.

the fourth and sixth columns the diameters of the variables that were required to be equal the standards in size. These data are more clearly shown in Fig. 2, which gives the results for every *O* in their proper spatial relation.

We consider first the results of the Main Experiment. In the first space order, the objects in the region of the optic disk are 0.01, 0.45, 0.25, and

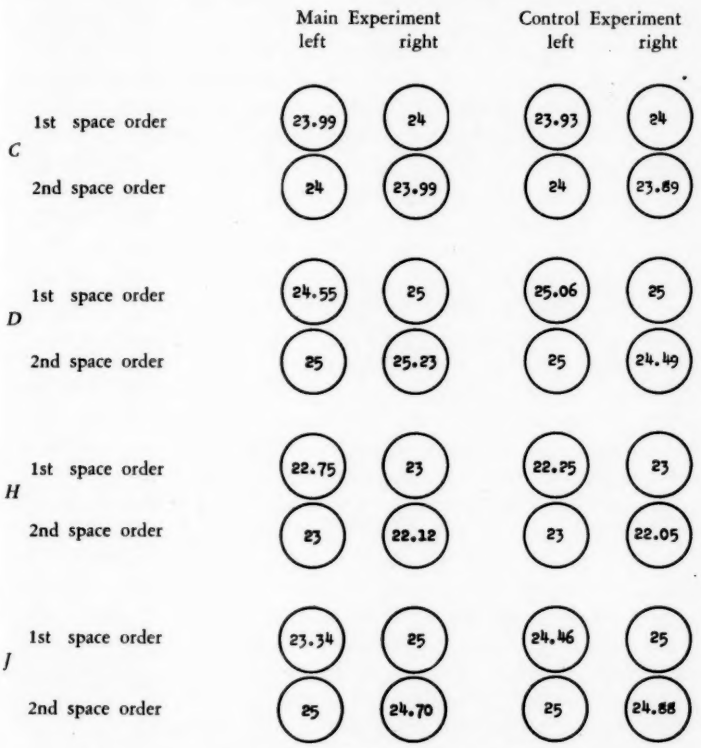

FIG. 2. LIMENS REPRESENTING POINT OF SUBJECTIVE EQUALITY

0.66 cm. *larger* for *C, D, H,* and *J* respectively than those in a symmetrically located retinal area. Not only are these differences small, but they are negated for 3 of our 4 *O*s, by the fact that in the second space order the objects in the region of the optic disks are *smaller* by 0.01, 0.88, and 0.30 cm. for *C, H,* and *J* respectively than the objects with which they were com-

pared. If the results of the first space order suggest contraction, then those of the second space order suggest expansion. Considered together, however, the results indicate *no change* at all. They merely show that these 3 *O*s were predisposed for some reason or other toward the variable. Why, we can only guess—perhaps *E's* movement in exposing the variable directed *O's* attention upon it—but in any case the results clearly indicate that, for these 3 *O*s at least, spatial perception in the region of the optic disk is not different from perception in a symmetrically located region of the retina. Had we not interchanged the positions of the standard and the variables, we might have concluded in favor of shrinkage or expansion, depending upon the space order used. Our experiment indicates, therefore, the futility of basing conclusions upon results obtained under a single set of conditions. The mere variation of space order gives different results and radically alters their interpretation.

For *D*, unlike the other *O*s, the objects in the region of the optic disk are larger in both space orders, by 0.45 cm. in the first and by 0.23 cm. in the second. But rather than inferring a contraction of the spatial values in the region of the optic disk, and appealing to individual differences, we are of the opinion, because of what is known of the spatial disposition of attention[38] and *D*'s disposition in particular,[39] that we are dealing here with a constant space error. We shall, however, return to this point later.

The results of the Control Experiment upon functional areas of the retina are similar. In the first space order, the variable (at the left) was 0.07, 0.75, and 0.54 cm. smaller for *C*, *H*, and *J* respectively than the standard, and in the second space order, the variable (now at the right) was 0.11, 0.95, and 0.12 cm. smaller. As in the Main Experiment, the variable was consistently smaller than the standard, showing again that these 3 *O*'s were predisposed toward it.

D's results in the Control Experiment stand again in contrast to those of the other *O*s, showing a *spatial* disposition that persists in both space orders. But now it is the object to the right of fixation, whether the standard or variable, that is smaller (0.06 and 0.51 cm. respectively for the first and second space orders). *O* is oppositely disposed in this experiment and in the Main Experiment, and thus some doubt is cast upon the interpretations drawn there. It suffices here merely to note the discrepancy.

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[38] K. M. Dallenbach, Position vs. intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-287; also R. S. Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left-handed observers, *ibid.*, 35, 1924, 267-269; A. M. White and K. M. Dallenbach, Position *vs.* intensity as a determinant of the attention of left-handed observers, *ibid.*, 44, 1932, 175-179.

[39] *D* has a pronounced attentional disposition to the left (Dallenbach, *op. cit.*, 284 f.).

We now compare the results of the two experiments. In order to eliminate the positional effects of the standard and the variables, the results of the two space orders were combined in each experiment. The composite limens, together with their measures of precision (h), σ values,[40] and reliability are given in Table III. This table does not disclose any striking differences between the comparisons made in the two experiments (0.09, 0.11, 0.28, and 0.14 cm. for *C, D, H,* and *J* respectively). Moreover, the differences lie, as shown in Table III under the caption "D," in opposite directions for the 4 *O*s: in the direction of the Main Experiment for *D* and *H,* and in the direction of the Control Experiment for *J*. *C* is practically balanced between the two experiments. All the differences, with the exception of *H*'s, are too small to be statistically reliable. *H*'s difference, on the other hand, is reliable, as the value obtained for D/σ diff. indicates mathematical certainty. *H*'s results, however, offer small comfort to those

TABLE III

SHOWING, FOR EVERY *O*, LIMENS AND MEASURES OF PRECISION IN EACH EXPERIMENT, DIFFERENCES BETWEEN LIMENS IN EACH, AND SIGNIFICANCE OF DIFFERENCES

O	St.	Main Experiment			Control Experiment			D	σ diff.	D/σ diff.
		L	*h*	σ	*L*	*h*	σ			
C	24	23.99	.67	1.06	23.91	.85	.83	+.0818	.0424	1.92
D	25	24.89	.62	1.14	24.78	.48	1.48	+.1090	.0591	1.84
H	23	22.43	.73	.97	22.15	.67	1.06	+.2815	.0447	6.29
J	25	25.52	.51	1.39	24.67	.51	1.38	−.1484	.0616	2.40

claiming anomalous spatial perception in the region of the optic disk, as they merely show that he compared more accurately at A and B (Fig. 1) than at C and D—a result that might have been expected, since the latter were slightly further removed from the center of fixation than the former.

D's results in the Main Experiment, which showed differences of 0.45 and 0.23 cm. in the two space orders in favor of the region of the optic disk, might be regarded as evidence of shrinkage—a larger object in the optic disk equaling a smaller one in a symmetrically placed retinal area—but we were of the opinion, as remarked above, that these results were due to a constant space error, to a disposition of this *O* toward the left. When we turn to the results of the Control Experiment, however, we find that this explanation will not suffice, because these results show a disposition in the contrary direction to the right—the object to the left now being larger than that to the right. A constant space error will not therefore explain *D*'s results. Neither, for that matter, will the hypothesis of anomalous spatial values in the region of the optic disk, for the difference between *D*'s liminal

[40] Calculated by the formula: $\sigma = 1/h(2)^{\frac{1}{2}}$.

values in the two experiments is, as shown in Table III, little better than chance. We are at a loss to explain *D*'s results, but explanation not involving the optic disk is not our immediate concern. *D*'s results are not significantly different in the two experiments, consequently we feel warranted in concluding that for him, as well as for the other 3 *O*s, spatial perception is not unlike in the region of the optic disk and in peripheral vision generally.

Summary and Conclusions

The results of this study may be briefly summarized. We found in the Main Experiment, in which spatial perception in the region of the optic disk was compared with perception in a symmetrically located area of the retina, a space error constant for one *O* and lying in the direction of the variable object for the other 3 *O*s.

Similar results were obtained in a Control Experiment, in which the spatial perception of two retinal areas symmetrically located outside of the blind spot was compared.

A comparison and an analysis of the results of these two experiments led us to the conclusion, in confirmation of Weber and of the experimenters who have followed him, that there is no difference in spatial perception between the region of the optic disk and the retinal periphery in general.

The suggestion is offered that the earlier discrepant conclusions may be due in large part to the fact that they were based upon results obtained under but one set of objective conditions.

THE DEPENDENCE OF AUDITORY EXPERIENCE UPON WAVE AMPLITUDE

By FORREST LEE DIMMICK, Hobart College

The vibratory motion which acts as the stimulus for sound may vary in frequency and in amplitude. The single attribute of pitch was held to parallel frequency and the single attribute of intensity to correspond to amplitude. When careful observation was brought to bear upon stimulus-frequency and its attribute, pitch, a number of psychological characteristics were found. Rich has given us an account of this expansion of 'pitch' into several qualitative attributes,[1] but no one has raised the question of the simplicity of the attribute of intensity. In view of what has happened to visual intensity,[2] it is apparent that this must be done. In the past, experiments that dealt with auditory quality used primarily periodic stimuli or tones, while most of the work on intensity avoided tones and used aperiodic stimuli or noises, such as falling balls or the sound pendulum.[3] To a large extent this has been due to the technical difficulties involved in measuring the stimulus. It is easy enough to determine the height of fall of a steel ball and to calculate the energy released as a function of that distance. To measure the amplitude and energy of a tuning fork or an organ pipe is a more difficult matter. Recent developments, however, in telephony and transmission engineering have put amplitude under precise control, and practical problems in those fields have formed the bases of several fundamentally psychological investigations. Unfortunately, however, the emphasis has been entirely upon the stimulus and the observational conditions have been neglected.

Fletcher in his recent book brings together many data that are of interest to psychologists. Some of them, however, can not be used as they stand because of the unusual experimental methods by which they were obtained. This is especially true of the values given for the minimum perceptible differences of sound intensity obtained in two different experiments with comparable methods, the second of which was carried on in the Bell Telephone Laboratories by R. R. Riesz apparently under Fletcher's direction.[4] In both investigations there was presented to the *O*s a pure tone

* Accepted for publication May 20, 1932. This study was made possible by a grant from the National Research Council for the purchase of apparatus.

[1] G. J. Rich, A Study of tonal attributes, this JOURNAL, 30, 1919, 121 ff.

[2] E. B. Titchener, Visual intensity, this JOURNAL, 34, 1923, 310 f.
C. Stumpf, *Die Attribute der Gesichtsempfindungen*, 1917.
L. T. Troland, *Psychobiology, II. Sensation*, 1930, 222 ff.

[3] Titchener, *Experimental Psychology. Instructors Manual, Quantitative*, 1905, 55 ff.
H. Fletcher, *Speech and Hearing*, 1929, 145.

[4] *Op. cit.*, 145 ff.

that could be made to fluctuate in intensity or to beat. Knudsen,[5] the earlier investigator, did this with a mechanical device which increased and decreased rythmically the resistance in the output circuit of a vacuum tube oscillator. The amount of rythmical change or Δ R was varied until it was determined at what point the resultant tone fluctuated just audibly. From the known constants in the circuit and the measured output of the receiver the root mean square (r.m.s.) pressure changes that could just be heard were calculated. Riesz extended Knudsen's work by introducing into the circuit with the standard frequency, a frequency differing by 3 d.v. per sec. from the standard, thus producing beats.[6] The voltage of the added frequency was adjusted to the point at which beating or fluctuation could just be heard. Fletcher describes Riesz' procedure as follows: "The minimum audible voltage of one attenuator, say B_1 (the standard tone) was determined, B_2 (the mistuned frequency) being set far below the minimum audible voltage. B_1 was then set at any desired voltage and B_2 adjusted until the observer signaled that he heard beats. The setting of B_2 was changed each time the observer signaled whether the tone seemed to fluctuate. After about 20 such adjustments the operator was able to locate with considerable certainty the setting of B_2 for which the observer was just able to detect a fluctuation in intensity."[7] From the minimum voltage of the mistuned frequency necessary to produce beats the change in the r.m.s. pressure in the ear was calculated as the intensive difference limen.

The use of this value for the intensive D.L. is a novel one. The implied physical argument that the only stimulus-change involved in the above procedures is that of intensity and that the perceptuation of fluctuation or beating depends upon a certain minimum degree of such stimulus change is obviously valid. On the other hand, the perception of fluctuation or beating is by no means a perception of simple sensory change in loudness. The intensive D.L. has always been taken to imply the perception of two discrete, successive sounds as different or as different in intensity. It is conceivable that the stimulus-difference necessary for the perception of fluctuation may prove to be the same as that for the perception of discrete difference, but that requires experimental demonstration.

In the following experiments we undertook to determine a group of discrete, intensive D.L. for tone by the usual psychophysical method for the purpose of comparison with and evaluation of the fluctuation D.L. cited by Fletcher.

Method and procedure. We used as a source of sound the output of a beat-frequency oscillator fed into a 12 in. dynamic loudspeaker. The electrical output of the oscillator and the sound output from the speaker were carefully checked for the presence of audible harmonics. The former was examined directly with a

[5] V. O. Knudsen, The sensibility of the ear to small differences of intensity and frequency, *Physical Rev.*, 21, 1923, 84.

[6] Fletcher, *op. cit.*, 146 ff.

[7] The fundamental difference between Knudsen's and Riesz' methods seems to be that, whereas the intensity change in a beating tone is relatively gradual, the fluctuations produced mechanically were abrupt.

cathode-ray oscilloscope and proved to be an almost perfect sine wave form. The tendency of the beat-frequency type of oscillator to drift or change in pitch was checked frequently while it was in use by reference to a standard frequency setting and never varied more than 2 d.v. in an experimental period. The wave form and the r.m.s. pressure given by the loud speaker for every value of voltage fed into it were observed and measured. A condenser microphone and amplifier calibrated to read in bars gave us the quantitative values and its electrical output as examined in the oscilloscope indicated that the speaker introduced no distortion of the original wave form. The speaker was situated in a sound-proof room with thickly padded walls which absorbed a high percentage of the sound that struck them so that the effect of standing waves was negligible at our stimulus-intensities. The *O* sat 3 m. in front of and facing the speaker. He maintained a constant position of his head during the presentation of stimuli. Using the method of constant stimulus differences, after the usual preliminaries, we determined the D.L. with 6 *O*s for a single frequency, 256 d.v., at 4 amplitudes.

Results. (1) Limens. Table I gives the absolute and relative D.L. in r.m.s. pressure (bars), in power (microwatts), and in the relative units of power ratio to the standard of 1 microwatt. For comparison, Reisz' values at the same pitch and the same intensities are put into the fourth section of the table.[8] In either unit, it is obvious that the limens Fletcher reports are much smaller than ours.[9] Our relative D.L., the ratios of the power increments to the standard powers, are more than twice as large as his. Such wide differences must be due to the difference in the observational problem of judging either intensive likeness or difference between two discrete tones, or the smooth or fluctuating character of a single tone. Certainly the differences between the two sets of results are so great as to be of importance in practical problems as well as in psychological. The parallel courses of the two curves shown in Fig. 1 indicate that the two types of data may be related and may possibly depend upon the same aspect of the auditory mechanism, but Fletcher's results cannot be taken as equivalent to the 'just noticeable difference' or intensive D.L. as that term is used and understood in psychology.

It is evident that although the absolute values of the D.L., either in bars pressure or in microwatts power, increase as the standards increase,

[8] Fletcher, *loc. cit.*

[9] It has become customary in transmission engineering to express sound intensities in units of their power ratios to a given standard. The usual standard is one microwatt. Since the literature contains values expressed in bars r.m.s. pressure and since Fletcher gives Riesz' relative D.L. in microwatts, we have tabulated our results in all three units. The various measures are related according to the following formulae: (1) $J = p^2/415$; (2) $\propto$ (db) $= 10 \log_{10} J$; (3) $\Delta\propto = 10 \log (1 + \Delta J/J)$. The values of $\Delta J/J$ and $\Delta\propto$ quoted from Riesz are calculated from his tables and formulae. Fletcher, *op. cit.*, 151.

the rate of increase does not follow the simple form of Weber's law.[10] This fact is even more apparent in the relative D.L. The ratios of the D.L. increments to their standards decrease rapidly as the standards increase. This same fact is noted by Fletcher in reference to Riesz' D.L. of fluctuation where it is further complicated by frequency.[11] The mathematical relation between sensation (S) and stimulus in microwatts (J) for our results[12] is expressed in the formula: $S = 7.2 \log (J^{.79} + 4.6)$. Because of the limited number of determinations upon which it is based,

TABLE I

D.L. OF AUDITORY INTENSITY AT 256 D.V.

Standard (r.m.s. pressure in bars)	p	.054	.09	.126	.174
	p+Δp	.0756	.1176	.1637	.2174
	Δp	.0216	.0276	.0377	.0436
	Δp/p	.40	.31	.30	.25
Standard (microwatts × 10^{-6})	J	7.03	19.52	38.26	72.95
	J+ΔJ	13.77	33.33	64.57	114.10
	ΔJ	6.74	13.81	26.31	41.15
	ΔJ/J	.96	.71	.69	.56
Standard (decibels-power ratio to 1 microwatt)	∝	−51	−47	−44	−41
	Δ∝	2.75	2.38	2.28	1.99
Standard in Sensation Level (db above minimum audible power)	S.L.	24	28	31	34
Riesz' D.L. of fluctuation calculated from his formulae					
Standard (microwatts × 10^{-6})	J	7.03	19.52	38.26	72.95
	ΔJ/J	.44	.34	.28	.27
	Δ∝	1.58	1.27	1.07	1.04

this formulation is offered only tentatively as suggestive of the general form of Weber's Law that may be applicable.

(2) Analysis of the intensive judgment. In the course of our experi-

[10] M. Guernsey, A Study of liminal sound intensities, this JOURNAL, 1922, 554. Guernsey made a number of determinations of j.n.d. ratios beginning at the lowest audible intensity. She concludes that "Weber's law as applied to audition apparently holds true with a fraction of about one-third throughout the middle range of intensities. The fraction is larger for low tones and for very high tones. The fraction is also larger near the limen." This seems to be in general agreement with Riesz and with us, but it is impossible to make a direct comparison of results since her's are given in terms of change of resistance in her circuit and were obtained, apparently, by a method of continuous change.

[11] Fletcher, *op. cit.*, 149.

[12] I am indebted to Professor Walter H. Durfee for the mathematical determination of the formulae.

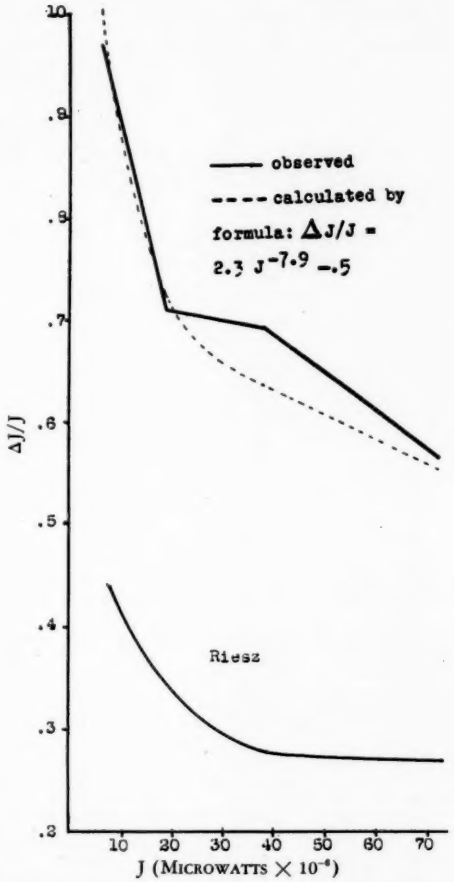

FIG. 1. THE RELATIVE D.L. AS A FUNCTION OF STIMULUS-INTENSITY AT 256 D.V. FROM RIESZ' DATA AND FROM OURS

ments we asked the *O*s to describe the experimental bases upon which they were making their judgments. From these reports it is evident that the difference between two tones is not as simple as, for example, that between a lighter and a darker gray. All *O*s agreed that there is some sort of more-less aspect in the tones that is obvious when the tones differ considerably, but that this is not the only mode of differentiating between

them, and further that it is not necessarily the basis of judgment when the difference between the tones is small. In such cases the judgment approaches one of just 'different' instead of 'different in intensity.' One *O* reported that the weaker tone is "smaller," "further away," "less tonal," "simpler," "shorter," "duller or lower." Another characterized the louder tone as "more spatial," "thicker," "not as distant," "clearer," "double," "well rounded," "higher in pitch." All the *O*s applied comparable terms to the tones and reported that when the members of a pair were only slightly different, one or more of the other aspects or criteria were used in making the judgment. In addition, visual and motor accompaniments were often present and invariably carried some stimulus reference such as, "you pushed harder for the first tone."

TABLE II

COMPARISON OF LIMENS OF INTENSITY AND OF VOLUME

(Values in bars p and microwatts $\times 10^{-6}$, J)

		Intensity	Volume	Volume/Intensity
	Δp	.037	.132	3.5
D	ΔJ	26.4	122.1	4.6
C	Δp	.043	.072	1.6
	ΔJ	30.7	56.2	1.8
M	Δp	.029	.082	2.8
	ΔJ	19.4	65.6	3.4

When we had completed the determination of the series of intensive limens, we selected three of the most common characterizations or secondary criteria; namely, volume, brightness, and pitch, and determined the limens for them with 3 *O*s, at a single intensity (0.126 bars; 38.3×10^{-6} microwatts; —44 db.). The *O*s had no difficulty in judging volume and gave much larger limens than for intensity. These judgments, therefore, are fundamentally different from those of intensity. The volume aspect which seemed to the *O*s as definite as intensity was characterized as "an impression of size," "total extensity," "number of elements." It seems safe to state, then, that volume changes with amplitude as well as intensity.[13]

The conclusions that may be drawn from the brightness D.L. are not so positive. Their magnitudes approximate those for volume so that the brightness judgments certainly are not the same as the intensity judgments. The reports of the *O*s indicate that brightness is probably different from

[13] H. M. Halverson, Tonal volume as a function of intensity, this JOURNAL, 35, 1924, 360. Our results are in close agreement with Halverson's though a quantitative comparison cannot be made since his and our sets of values cannot be reduced to the same units.

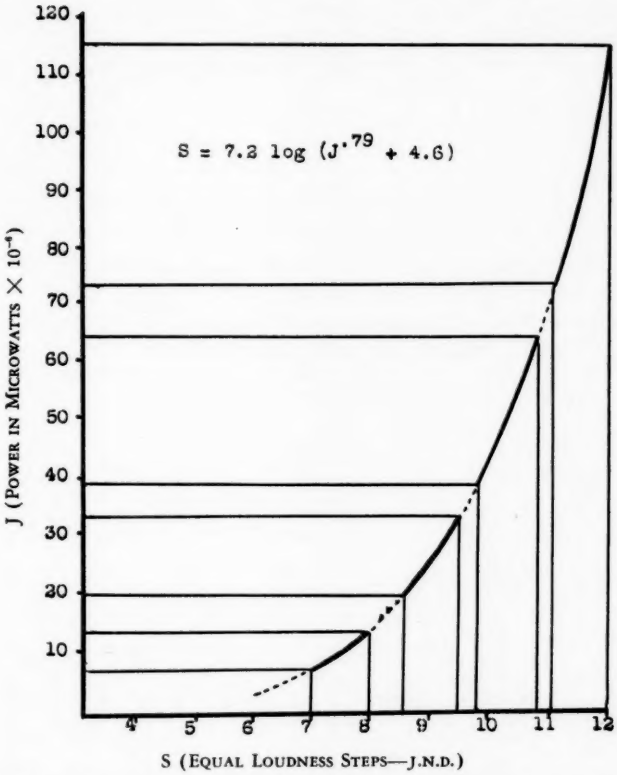

FIG. 2. SENSORY-LOUDNESS AS A FUNCTION OF STIMULUS-INTENSITY
(The solid parts of the curve represent the observed j.n.d.)

volume as well. Bright tones are "clear," "sharp," "pointed," "shrill," while dull tones are "unclear," "flat," and lack "sharpness."

The determination of pitch D.L. proved impossible. While all *O*s insisted that there were frequent changes in pitch, often so great as to obscure the intensity change, they showed no close functional relation to the stimulus-intensities, though there appeared a general tendency for louder tones to be heard as higher.[14]

[14] W. R. Miles, Accuracy of the voice in simple pitch singing, *Psychol. Monog.*, 16, 1914, (no. 69), 13-66. Miles noted the tendency of a singer to reproduce a more intense tone as higher in pitch than the same tone sounded more weakly.

SUMMARY

Our experimental results show four things.

(1) The "minimum perceptible differences" given by Fletcher are not the same as the usual intensive D.L.

(2) It is apparent that for the determinations we made the simple form of Weber's Law does not hold. Our results give the formula, $S = 7.2 \log (R^{.7979} + 4.6)$.

(3) The assumed one to one relation between physical amplitude and psychological intensity is too simple to be adequate. Changes in wave amplitude produce changes in auditory volume, brightness and pitch, as well as in intensity.

(4) Intensity judgments themselves are perceptual and, unless great care is taken in control of the observational setting, may depend so much upon secondary criteria that they do not reflect accurately either psychological intensity or physical amplitude.

INSTRUCTION AS A FACTOR IN 'INCIDENTAL' LEARNING

By JOHN G. JENKINS, Cornell University

The problem of 'incidental' learning—that is to say, learning which occurs in the absence of a specific intent to remember—has undergone an extraordinarily large amount of experimental investigation during the past fifty years. Although Myers[1] reports that the first experiments were conducted by Cattell in 1893, we find that Ebbinghaus was aware of the existence of the problem early in the preceding decade,[2] and that he attempted to maintain a constant attitude towards his nonsense syllables during his investigation. Since all of the researches on the psychology of testimony had been regarded as significant for this problem, the experimental literature contained more than one hundred titles by 1913,[3] and at least fifty more have been added during the two succeeding decades. These studies have shown such close agreement that Meumann was able to state that "if we have the will to imprint the observed data upon memory in order that we might subsequently be able to reproduce them, we actually retain them more readily; and when the will to remember is lacking, the incorporation into memory fails to take place, or if it does occur, it is a matter of sheer accident."[4]

A study of the literature reveals, however, such marked discrepancies between the results of the various investigators that one is led to accept Bergson's dictum that incidental memory is a 'capricious' affair. In a recent study, Shellow has taken note of this apparent capriciousness and has collected evidence which leads her to conclude that "incidental memory is only 'capricious' when we do not know its determining factors, but once these are analyzed and the laws governing them formulated by experimental research, we find incidental memory as logical and as inevitable as the better accredited form of direct memory."[5] Her contribution to our knowledge of the determining factors is summarized in one sentence which points out that the materials retained in incidental memory are those "which had become fixated because their own intensity was sufficient to cause fixation, or because they had in some way become linked up with

* Accepted for publication November 21, 1931.

[1] G. C. Myers, A study in incidental memory, *Arch. Psychol.*, 4, 1913, (no. 26), 3.

[2] H. Ebbinghaus, *Memory*, Eng. trans. H. A. Ruger and C. E. Bussenius, 1913, 11.

[3] Myers, *op. cit.*

[4] E. Meumann, *The Psychology of Learning*, Eng. trans. J. W. Baird, 1913, 74.

[5] S. M. Shellow, Individual differences in incidental memory, *Arch. Psychol.*, 10, 1923, (no. 64), 75.

the individual's habitual interests and past experience, and had become incorporated with them."[6] The present study is designed not so much to add further evidence to a mass of experimental data already bulky as to attempt a search for additional determining factors with a view to reducing still further the apparent capriciousness of incidental memory. To this end a standard technique was employed, patterned closely after the conventional methods of many of the earlier investigations. Since the most critical technique seemed to be that of comparing the scores made by the same individuals with and without the intent to learn and to remember, the method was shaped accordingly.

Procedure. One individual sat in a small booth. Through a window before him he had a clear view of the drum of a Spindler and Hoyer memory apparatus as it revolved before a large reading-glass under constant illumination. Outside the booth a second observer sat in such a position that the syllables on the drum were invisible to him; he then prepared deliberately to learn the syllables as they were read to him by the first observer.[7] When the two *O*s were in position, the following common instructions were read aloud to them.

You are to take part in an experimental investigation of auditory memory in which an attempt is to be made to determine the effectiveness of memorizing from the sound of the syllables.

To anyone trained in psychological research, the vacuity of this statement would have been obvious, and suspicion would undoubtedly have been aroused. Since untrained undergraduates were to serve as *O*s, it seemed better, however, to attempt some such simple deception, rather than to tell them nothing of the nature of the problem and so to leave them at the mercy of the first guess that appeared on the scene. The success of this procedure will be apparent presently; we may remark now that all *O*s accepted our explanation in good faith and did not indulge in further speculation as to the purpose of the investigation.

The *O* who was to read aloud the syllables without intent to learn them (hereafter referred to as the 'reader' (*R*), took his seat in the booth and received the following additional instruction.

Read each syllable clearly and distinctly as it appears in the window. Pronounce all vowels as short. Remember that the success of the experiment depends upon the accuracy of your reading.

The last part of the instruction was given with a view to reducing still further the probability that this *O* would attempt to learn the series. The other *O*—who was to make a deliberate attempt to learn, and who will be called hereafter the 'memorizer' (*M*)—took up his position outside the booth and was further instructed as follows.

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[6] *Ibid.*, 75.

[7] Two undergraduates, Mr. S. B. Schreuder and Mr. G. C. Zahn, secured the observers, read the instructions, and collected the reports. Without their faithful and painstaking efforts this study would not have been possible, and the author wishes fully to acknowledge his debt of gratitude to them.

Listen carefully to the syllables as they are read. Make every effort to learn them as rapidly as possible. Try to anticipate each syllable before it is read. When you can do this for each syllable, have the reader stop while you repeat the series aloud. If you fail on the first trial, the reading will be continued until you are successful.

The syllables were now exposed (at a rate of 22 sec. for the 20 syllables) and read aloud by *R*, with a brief pause at the end of each series. When *M* had made a single successful reproduction of the series, both observers were released with the request that they return to the laboratory after 24 hours "to complete the learning." When they were seated in their proper places after a lapse of 24 hr., the following instructions were read to them.

Write down on the slip of paper every one of the syllables you can remember. Try as hard as you can and spend as much time as is necessary.

In addition, use the rest of the sheet of paper to tell all you can about your experiences in learning and in reproducing. Describe your attitudes and tell about any cues or other devices you may have used either in the memorizing or in the reproduction. Any descriptive comment you may make, no matter how irrelevant it may seem, will be of value to the experimenter. Above all, do not discuss this experiment with anyone. Its future success depends absolutely upon the maintenance of secrecy regarding the procedure as observers will be chosen from all parts of the student body.

The success of this and of our earlier precautions is indicated by the absence of any foreknowledge on the part of our *O*s. None of them had been forewarned of the actual purpose of the investigation; all *R*s expressed surprise and not a few indignation at being asked to recite upon what they had never been instructed to learn.

Since the procedure called for a comparison of the scores made by the same individual observing with and without the intent to remember, the person who had acted as *R* was now drafted to serve as *M*, while a new *O* took his place in the booth to read aloud a new series of syllables, unaware that he would be asked later to reproduce the syllables he had been reading. Data will be presented on 24 individuals who served in the dual capacity as *R* and *M*. Thus we shall be able to compare at any time the scores of the same individual as he observed under the two sets of instructions, and we shall not be compelled to limit ourselves to gross comparisons of groups.

Results

An initial glance at the results (Table I) will show that our observers when acting as *R* (and hence free, as we presume, from any intent to learn) recalled on the average only 10.8 ± 3.6 syllables, while the same individuals recalled 15.9 ± 2.4 syllables when they were serving as *M* and making definite efforts to learn the series. Now this result, taken in its gross form, is in complete agreement with the common outcome of the earlier investigations in indicating that incidental memory is definitely inferior to deliberate intentional memory. Indeed we find no evidence at all of the alleged 'capriciousness' of incidental memory until we turn to the detailed consideration of the results for each individual observer. This information is supplied in Table I.

Granting that individuals will vary widely in retentiveness, the differences between the reports of the various observers are at least as striking as the general trend of the data recorded. Thus we note immediately that two *O*s actually reported *more* syllables when acting as *R* than they did as *M*, and one more who recalled the same number under the two attitudes, while the other *O*s vary widely in the differences to be noted between the two situations. This appears at first sight to be indeed a capricious and irregular affair, and, without the reports from the *O*s, it would be likely to remain so. The first clue to an explanation was discovered in a report made

TABLE I

SHOWING THE NUMBER OF SYLLABLES RECALLED BY EACH *O* WHILE SERVING AS *M* AND AS *R*

(*O*s in the first column report that they made no attempt to learn as *R*)

O	Syllables recalled as		*O*	Syllables recalled as		*O*	Syllables recalled as	
	R	*M*		*R*	*M*		*R*	*M*
An	6	18	*Ar*	13	11	*Mo*	20*	16*
Ed	8	17	*Bo*	9*	13*	*No*	18*	19*
Ko	7	14	*De*	9	12	*Po*	9*	20*
La	7	13	*Fu*	8	15	*Pr*	14*	16*
Li	6	15	*Gl*	11*	16*	*Sa*	12*	13*
Mi	14	18	*Ho*	9	15	*Sc*	20*	20*
Ry	10	18	*Ka*	6*	17*	*Sz*	14	19
Tr	6	10	*Mc*	15*	19*	*Wi*	9	18
Av. = (Col. 1)	8.0	15.4				Av. = (Cols. 2 & 3)	12.3	16.2
A.D.	2.0	2.4				A.D.	3.5	2.3
			Av. of all =	10.8	15.9			
			A.D.	3.6	2.4			

by one *O* in the early stages of the investigation. This *O*, while acting as *R* and while presumably free from any intent to learn—if the assumption of earlier investigators is valid—reported in part as follows:

"After I had mastered the pronunciation, the experiment began to get a bit boring. So, since X looked as if he'd be all afternoon in learning, I tried memorizing them myself . . . I had learned them before X made his first attempt to recite them."

Here, then, is an *O* who, in spite of the fact that we were employing conditions established as standard for freedom from intent to remember in earlier researches, had actually embarked upon a deliberate attempt to memorize the syllables. Her report indicates that she is furnishing *not* a comparison of incidental learning and intentional learning, but simply a review of two cases of deliberate memorizing. Since we should never have suspected this without her report on attitude, it becomes imperative that we search the reports of other observers to discover whether any other subject has violated the major premise of the experimental conditions.

This search is, unfortunately, not as simple a problem as it would appear at the outset, for an organism is instructed, as Bentley has pointed out,[8] not alone by directions issued to it by someone else (formal instruction) but as well by itself (self-instruction) and by the occasion or situation confronting it (occasional instruction). The case we have just presented is obviously one in which the organism was acting under self-instruction; and we shall be concerned presently with numerous other examples of this sort. Meanwhile the reports must be searched for traces of occasional instruction. Since it is common knowledge that nonsense syllables are by no means 'nonsense,' and since everyone will grant that an untrained *O* will endow these syllables with meaning even if given specific instructions to the contrary, this aspect of the problem scarcely requires special mention.[9] This is, however, only one form of occasional instruction. The reports of our observers present much evidence to show that the occasion was constantly prompting the subject in other ways to remember this or that syllable as he read the series aloud. Thus one syllable called for memorization because it was difficult to pronounce; a second, because it indicated that a pause was to follow; while a third was persistently missed by the *M*. These examples and many others contribute to the notable fact that not a single *O* failed to indicate occasional instruction to learn certain of the syllables at a time when he was presumably free from any such instruction.

Moreover, if we concern ourselves for the moment only with self-instruction and scrutinize in this light the reports available, another noteworthy fact comes into view. Out of the 24 individuals with whom this study is concerned, 10 make definite, positive statements to the effect that they made deliberate attempts to learn *while acting as R*, and, according to the fundamental assumption of this traditional method, while they were wanting in any such intention. (It must be recalled that this report of occasional and self-instruction to learn comes to light in every instance without the aid of any question more specific than those contained in the general instruction to report upon attitude.)

It is possible to test roughly the significance of these reports on instruction by considering briefly the contrary and doubtful cases in Table I in the light of the *Os*', protocols. It is found that, with but a single exception (*Ar*), those who report as many (or more) syllables while acting as *R* state definitely that they made deliberate attempts to learn the series. Those who reported self-instruction to learn are indicated by asterisks after the number of syllables recalled.

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[8] M. Bentley, *The Field of Psychology*, 1924, 384-396.

[9] Cf. G. L. Freeman, The rôle of context in associative formation, this JOURNAL, 42, 1930, 173-212.

Sc (who reported 20 syllables under each attitude) states simply that "I attempted to learn in groups of four . . . and was able to say them over before *M* had learned them." *Mo* (who recalls 20 as *R* and 16 as *M*) begins his report with the statement "I attempted to learn the list," and tells in detail how he went about the process. *No* (18-19) and *Sa* (12-13) describe the way in which they went about memorizing while reading. *Po* notes with regret that he did not have time as *R* to memorize all of the syllables, which may account for the fact that he recalled only 9 in that capacity, whereas he retained 20 as *M*. *Gl* (11-16), *Ka* (6-17), *Pr* (14-16), and *Bo* (9-13) report 'half-hearted' or occasional attempts to learn. We cannot tell how far their results are affected by this amount of self-instruction, but we may be certain that the results of these *O*s do not belong among data representing the facts of 'incidental' learning. Finally we must exclude the results of *Mc* since he remarks innocently that he tried to learn the series by associating each syllable with its neighbor.

The case of *Ar* appears at first sight to fall badly out of line with our general conclusions, for she reports 13 syllables as *R* and only 11 as *M*, although she insists that she made no effort to learn during the former case. Even though she reports a wealth of occasional instruction while acting as *R*, her report as *M* provides a definite clue for the smaller number of syllables recalled under definite instructions to learn. Scrutiny of her report indicates that she made up an intricate mnemotechnical sentence for remembering the syllables as they were read to her. She repeats this in detail and succeeds in remembering 20 syllables with its aid (whereas she had remembered but 14 when acting as *R*). Her report shows, however, that she has warped the syllables to fit the scheme in the former situation. The result is that 9 of the 20 syllables recalled as *M* are incorrect, while only one of the 14 remembered as *R* (without mnemomic aid) is incorrect. On closer inspection, then, her case appears to be one of special difficulty, rather than of opposition to the general trend we have noted.

We have no way of knowing the extent to which self- and occasional instruction cut across the results obtained by earlier investigators; but there is no reason to suspect that these forms of instruction were less prevalent or less important in their studies than in ours. Realizing that such a small number of cases can have no final quantitative value, we may nevertheless search our own records for cases in which the observers state definitely that they did *not* attempt to learn. We find 8 who say that they made no attempt to memorize. If we isolate their results (as has been done in the first column of Table I) we find that they recalled only 8.0 $\pm$ 2.0 syllables while serving as *R* (compared with 12.3 for the rest of the group) while, as *M*, the number of syllables retained remains practically unchanged (15.4 $\pm$ 2.4 compared with 16.2 for the rest of the group). Even this decrease is, of course, not significant of results for the uninstructed organism, since everyone of these *O*s reported occasional instruction to learn, even though there was no self-instruction to this effect.

SUMMARY

The results of this study are two. (1) Further experimental support is given to a result often verified in the earlier experimental literature; that

is, that 'incidental' learning appears to be markedly inferior to intentional learning. It is shown, however, that, in this investigation at least, the gross results are diluted by the presence of data from subjects who were self-instructed to learn in a situation which supposedly involved nothing but incidental learning.[10] The reports of the observers contain a further suggestion that occasional instruction (instruction arising from the stimulus-situation in some of its aspects) may also have cut across and modified the result in every case. It is obvious that the conditions reported by Shellow may conveniently be reported under the general heading of occasional instruction. Since the general arrangement employed here is closely analogous to that used in many of the earlier researches, there is a strong suggestion that these researches may have been subject to similar dilution. This would imply that the difference between incidental and intentional learning may be much larger than has been supposed.

(2) The second, and perhaps the more important, result of this study is its indication that post-experimental reports from observers are vital necessities whenever the interpretation of the results hinges upon the attitude assumed by the observer during the experiment. Weber and Bentley have pointed to the importance of occasional and self-instruction,[11] and Anderson's experiments on attitude have led him to conclude that "specific instruction, designed as a means of assuring constancy of attitude and performance in experimentation, is by no means adequate even where observers are highly trained and carefully instructed. As an approach to an adequate control of attitudinal conditions it appears that frequent determinations of attitude must be made throughout any series of observations . . . and that self- and occasional instructions have quite as much to be taken into account as determinants of our psychological performance as have verbal commands of the formal kind."[12] Until such post-experimental reports on attitude are given, one may well be hesitant in accepting results obtained under the tacit assumption that the observers were successful in making and maintaining a critical shift in attitude demanded by the experimenter.

[10] During a preliminary presentation of this report at a sectional meeting of psychologists one member of the group arose to suggest that all of our trouble would have been avoided if we had instructed our observers *not* to learn the syllables when they were serving at readers. His faith that negative formal instruction would succeed where positive formal instruction had failed and that it would cancel all self- and occasional instruction is worthy of report.

[11] O. F. Weber and M. Bentley, The relation of 'instruction' to the psychosomatic functions, *Psychol. Monog.*, 35, 1926, (no. 163), 15.

[12] O. D. Anderson, An experimental study of observational attitudes, this JOURNAL, 42, 1930, 368 f.

THE INFLUENCE OF ELECTRIC SHOCK IN MIRROR TRACING

By M. C. Barlow, University of Utah

Few studies in the field of motivation have been primarily concerned with the effect on learning of electrical shocks (pain) used as punishment for errors. Those that have touched upon this problem, moreover, employed methods and apparatus that have inadequately controlled the essential variables of the learning process. So as to escape the methodological errors of these former studies, the writer devised an apparatus and a technique that gave reliable measures of the speed and accuracy of learning. Specifically, the problem was to measure the influence of electrical shock upon the learning of a complex motor skill—mirror tracing of a 6-pointed star.

Procedure

Method. The problem demands complicated apparatus, which has the special merit of making a continuous record of 4 variables at a time.[1] The records are made on waxed paper drawn over a revolving drum from two movable spools. Four telegraph sounders of low resistance, equipped with sharpened pieces of piano-wire fastened to the movable arms so as to serve as markers, were set in such a way that the markers stood in line on the moving paper. When *S* closed the different electric circuits, the markers which might register 5 or 6 responses each per sec., broke lines on the paper. A seconds pendulum operated one of the markers. Another of the markers was connected electrically to metal plates a, a_1, a_2, etc., in the non-conducting floor of the track of Fig. 1. The third marker was connected to 3 consecutive rays on the outside of the track, in series with the 3, in order, on the opposite side, but on the inside of the star, that is 1, 2, 3, 4P, 5P, and 6P. These were always the non-shock sides. Similarly, the shock sides were 1P, 2P, 3P, 4, 5, and 6. An AC variable speed motor of 1500 r.p.m. and a speed reducer with a ratio of 1 to 100, pulled the waxed paper under the markers. The shock was inflicted automatically through electrodes on the wrist of the *S*, when the stylus touched the shock-side of the pathway. It continued until he moved the stylus away. The shock was delivered from a lighting circuit of 110 volts which passed through a transformer with a maximum of 10,000 volts, and a rectifying tube, the filament of which was heated by the current from a toy transformer connected in series to the same lighting system. The temperature of the filament controlling the amount of DC current was regulated by a variable resistance coil of 10 ohms.

The *S*s, during the experiment, sat behind a screen, tracing with a stylus the brass star shown in Fig. 1. The greatest diagonal width of the star is 7 in.; the open pathway between the notched sides is ½ in.; the tracing-stylus, ¼ in. in diam. at the base. The stylus has a thin plate-like collar of 1-in. diam., which pre-

* Accepted for publication October 22, 1931.

[1] For a description and diagram of the apparatus cf. *Popular Mechanics*, 55, 1931, (no. 2), 208.

vents *S*'s fingers from touching the metal part of the stylus which conducts the electric current. The shock continued only during the contact of the stylus on the shock-side of the star.

Subjects. The *S*s were selected at random from a class in introductory psychology. In the experimental group, Group I, there were 23 students; and in the control group, Group II, there were 30.

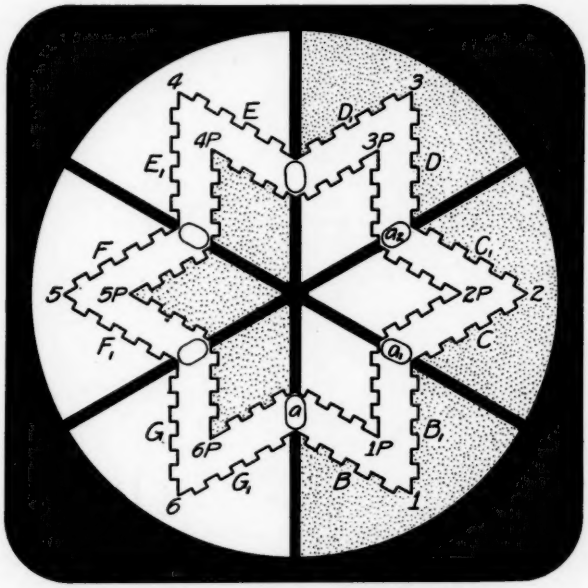

FIG. 1. DIAGRAM OF THE TRACING-STAR
The star, made of brass, is mounted on a bakelite base. The inside and outside track of every one of the 6 rays are insulated separately and connected by underposts in any combination desired.

Instruction. The *S*s of group I were given the following instructions:

The metal plate before you has an open pathway shaped like a star. With a pencil-like stylus you are to follow this pathway as you see it in the mirror by looking over the cape which prevents you from seeing the star directly. Trace the groove until you can move continuously around without coming into contact with the brass edges on either side. When you touch a side you may see a spark and hear a sound. The spark will not injure you, but you will receive a harmless electric shock when the stylus touches the inside notches on the right half of the pathway, and the outside notches on the left half. Do your best always; errors will count against you. The starting point is the vertex of the angle of the inner star that appears farthest from you. Move through the pathway with the maximum of speed which is favorable for the quickest elimination of all errors. If you get tired, or

restless, stop a short time; but you are to begin again where you leave the path. In case you jerk the stylus out of the trail return where you leave it. Remember, the task is to learn to move around and around the star without touching the sides. Put the stylus in place and begin when I say "Go."

The *Ss* of Group II were given the same instructions except that the part referring to the electrical shocks was omitted. The strength of the shocks was 0.5 milliampere at 10,000 volts. All the *Ss* made 20 circuits of the star.

RESULTS

(1) Speed. Infliction of pain by electrical shocks as punishment for errors slows down the rate of learning. Table I shows the average time taken by the *Ss* in completing the 20 circuits of the star.

TABLE I

SHOWING FOR EACH GROUP OF *Ss* THE AVERAGE TIME (IN SEC.) TAKEN IN MAKING 20 CIRCUITS OF THE STAR, AND THE AVERAGE NUMBER OF ERRORS ON THE SHOCK AND NON-SHOCK SIDES

Group	Time		Errors	
	Av.	S.D.	Shock side	Non-shock side
I	1202.8	433.0	175.9	238.2
II	990.2	358.7	286.3	302.2

The average for Group I is 1202.8 sec., S.D. 433. The best score was 572 sec., the worst, 2091 sec. The average for Group II was 990.2 sec., S.D. 358.6; the best score was 360, and the poorest, 1831. The significance of the difference between the records of the two groups of *Ss* is 2.88, which is large enough to be considered reliable. Punishment for errors by electrical shock increased the time of the performance by 21.5%.

The above results agree with those of Dodson who found that rats acquired more time to learn a two-part discrimination box when pain was used as a motive than when hunger was the spur.[2] They stand, however, in disagreement with the findings of Bunch, in which electrical shock applied as punishment for errors decreased the learning time by 30%.[3]

Certain comments made by the learners suggest that learning with punishment for errors resembles the case of learning with caution or threat as a warning. The *Ss*, while avoiding the shocks, would ofttimes verbalize as follows: "Must not touch that side; no, not that; look out for shocks; or danger there; this side is safe; and it doesn't shock here." Incipient movements, accompanying such responses, would bring the hand to a stop just in time to avoid a shock. Hence, as a result of inhibitions, it took more time to make one round on the star, and there were fewer errors.

[2] J. D. Dodson, Relative values of reward and punishment in habit formation, *Psychobiol.*, 1, 1927, 231-276.

[3] M. E. Bunch, The effect of electric shock as punishment of errors in human maze learning, *J. Comp. Psychol.*, 8, 1928, 343-359.

It is generally understood that both caution and warning exercise an inhibitory effect on speed as well as errors. This, however, is a point for later investigation.

The reliability coefficient of 0.913 $\pm$ .03 was calculated from the paired odd- and even-numbered time scores of Group I; similarly, 0.787 $\pm$ .04

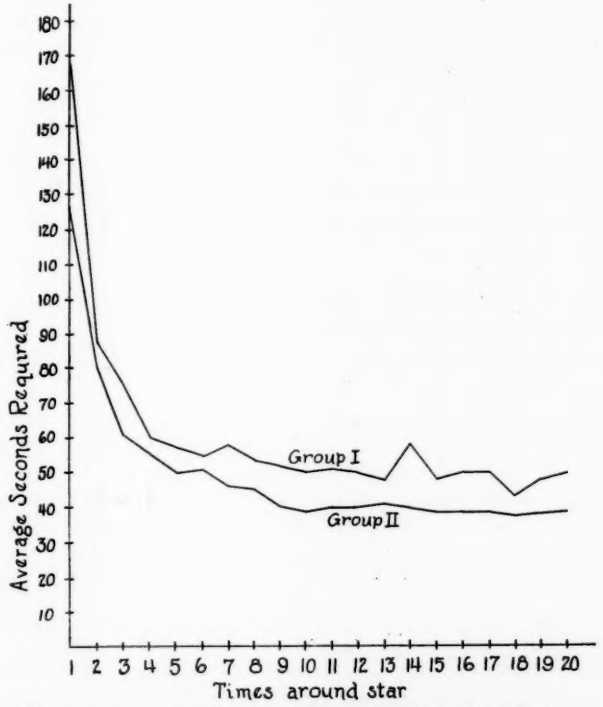

FIG. 2. SHOWING THE AVERAGE TIME REQUIRED BY EACH GROUP OF *Ss* FOR EVERY ONE OF THE 20 SUCCESSIVE TRACINGS OF THE STAR

Group I learned under punishment; Group II without punishment.

for Group II. The method of averages in the calculation tends to make the coefficients higher for the reason that individual irregularities are averaged. Paired odd and even circuits by individuals obviously would yield coefficients lower than those given above.

Average time in seconds is plotted against successive trials in Fig. 2. It can be seen at a glance that the average time of Group I is greater than

Group II for every trial. The graphs differ markedly in two respects, leaving out of consideration the first trial. Graph I descends with less steepness and straightens out with less flatness. The difference between the two mean scores in trial 1 is 40.4 sec. The average difference for trials 2-6 is 7.5 sec.; of trials 7-20, 10.9 sec. The punishment seemed to affect learning most during the early and late practice periods; and least during the intermediate stages, during which time rapid progress takes place.

Such an influence on learning cannot be accounted for in terms of emotional disturbance during the first few circuits of the star, since most of the *Ss* reported that their apprehension gradually wore off and practically disappeared toward the end of the sitting. Two *Ss*, however, were emotional throughout the performance, and a third noticed emotion only through the later circuits. In general, there was more emotion during the first half of the practice and less during the last. If emotion had served to slow down speed, the most marked effect would have taken place while the excitement was most stirring; but the reverse is true with the exception of trial 1, that is, the most effective influence of punishment on learning occurred during the stages in which there was least excitement.

The analysis of time scores in mastering the sides of the star, brings up the three problems; namely, difficulty of the sides, the place of serial order in learning, and transference of training. The same questions arise with both groups, as may be seen by the graphs of Fig. 3 in which seconds are plotted against successive sides of the star. The order in degree of difficulty for both groups is the same. The fan-like spread of the time-scores suggests temporal order, and position of the sides in the series as external factors which make for difference in amount of influence exerted by the shocks. Whether or not these are real factors or spurious ones remains to be proved. The tracing began from a point on the track directly in front on the side nearest *S*, and continued counter-clockwise around the star. Since most of the *Ss* were right handed it is possible that the direction of movement had something to do with the difficulty of the various sides as well as transfer. In order to establish the apparent fact of primacy in acquiring visual-motor skill it will be necessary to obtain further data with 'serial position' and 'transfer' controlled.

The results of the present study thus far agree with the findings of Dodson, viz., that learning with errors punished, took more time than learning errors without punishment. Contrary to this, both Johanson[4] and

[4] A. M. Johanson, The influence of incentive and punishment on reaction-time, *Arch. Psychol.*, 8, 1922, (no. 54), 1-53.

Vaughn[5] concluded that reaction-time decreased with electric shock administered as punishment for errors. The last conclusion might seem a contradiction to the others, but they all agree from the standpoint that the

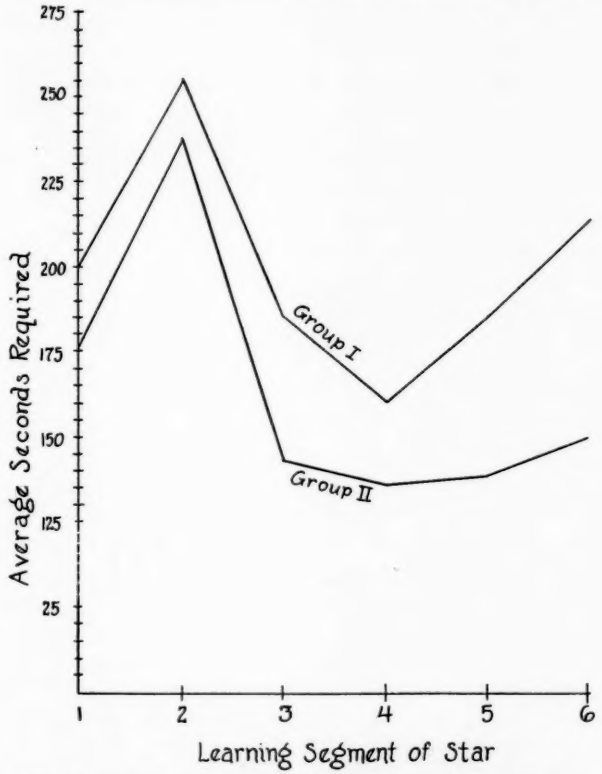

FIG. 3. SHOWING THE AVERAGE TIME REQUIRED BY EACH GROUP OF *Ss* TO LEARN EVERY STAR-SEGMENT
Group I learned under punishment; Group II without punishment.

infliction of pain during learning always brings about responses aimed at avoiding punishment. Sometimes the action is slower, sometimes faster, all depending on whether or not pain is thereby avoided.

[5] James Vaughn, Positive vs. negative instruction, *Publ. Nat. Bur. Casualty & Security Underwriters*, 2, 1928, 1-172.

(2) Accuracy. Errors were mostly of the visual-motor type resulting from the disruption of previously established coördinations. As *S* looked into the mirror he began the test by following the pathway over the sections in order from 1–6 (cf. Fig. 1); *i.e.* counterclockwise from a to a. The transverse movements across the track were due to the illusion of the hand seeming to move forward, whereas it was actually moving backward, the converse also being true. While passing over sections B_1 and D, moving counterclockwise, the hand seemed to be approaching the body although it was actually receding; along C and G_1 it looked to be going toward the right and nearer himself, but the motion was toward the right and farther away. Misperception of motion in two directions occurred on every ray, the illusion in one direction held for one side, while the illusion of another direction held for the other side.

TABLE II

STAR-TRACING ERRORS BY GROUPS I AND II

(Average errors for each trial)

Group	Side	Trial						Total
		1	2	3	4	5	6	
I	Non-shock	36.1	75.8	36.2	31.5	34.8	23.8	238.2
	Shock	25.2	35.6	17.6	17.17	32.6	47.0	175.7
II	Non-shock	52.1	84.8	43.0	53.0	42.4	26.9	302.2
	Shock	44.3	68.5	34.3	28.6	48.0	62.6	286.3

Punishment while learning progressed, reduced the number of errors. The average number of errors made per round by Group I was 414.13, with an S.D. of 288.1. Similarly, the average of Group II was 590, S.D., 234.3. A difference between the two mean scores of 2.75 times the sigma difference indicates that Group I are reliably more accurate than Group II. Those given electric shock on account of mistakes, learned with 30% less errors than the *S*s did who were not shocked. The punished learners made fewer mistakes on all segments except the first, cf. Fig. 4.

The segments of the star shown in Fig. 1 were connected as described above to inflict a shock for errors from the inside edges of one side and the outside edges of the other side of the pathway; a punishment edge being on one side with a non-punishment edge opposite in every segment. Contacts on the non-punishment side are designated non-punishment errors; on the punishment side, shock, or punishment errors. These errors for both groups of *S*s are represented in Table II.

In Fig. 4 are represented the average errors made on each circuit. Group I is consistently more accurate than Group II with the exception of trail 1.

The *Ss* of Group I showed a tendency to be more accurate on the shock sides than Group II; by making an average of 175.5 errors per trial, as compared to 286.3; the latter group did not receive any shocks from the

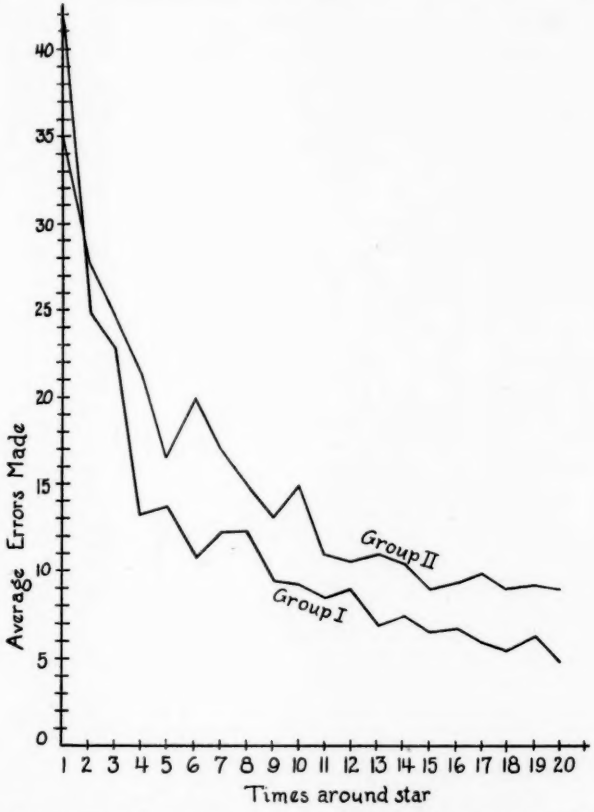

FIG. 4. SHOWING THE AVERAGE NUMBER OF ERRORS MADE BY EACH GROUP OF *Ss* IN EVERY ONE OF THE 20 SUCCESSIVE TRACINGS OF THE STAR
Group I learned under punishment; Group II without punishment.

shock sides. Also, Group I manifested greater accuracy on the non-shock sides, with an average of 238.2 mistakes per round as against 302.2, in spite of the fact that the former Group did not get any shocks from the non-shock sides. Evidently, the *Ss* of Group I did not, while avoiding the

shocks, hug the non-shock sides. Accuracy resulting from the shocks was transferred to non-shock performances. The sigma difference of the two means of errors made on the non-shock sides, expressed in difference units

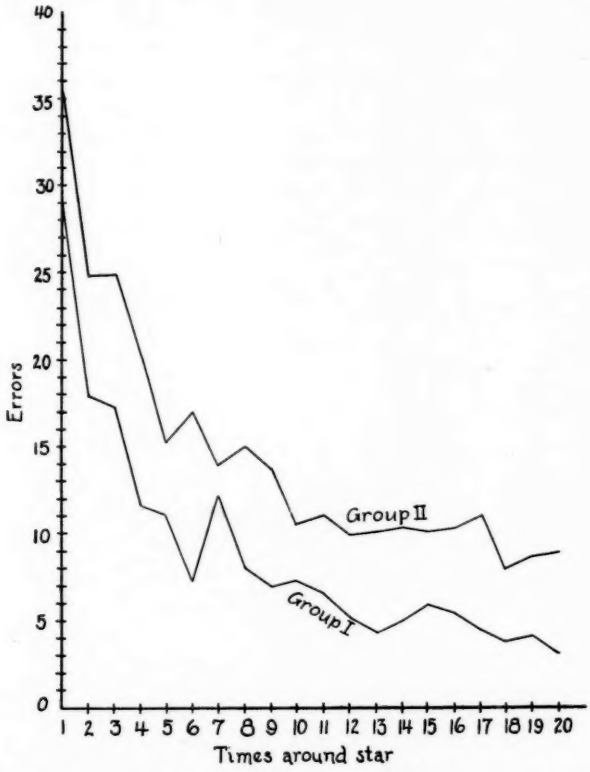

FIG. 5. SHOWING THE AVERAGE NUMBER OF NON-PUNISHMENT ERRORS OF EACH GROUP IN EVERY ONE OF THE 20 SUCCESSIVE TRACINGS OF THE STAR

Group I, curve of non-punishment errors by group learning under punishment; Group II, curve of non-punishment errors by control group, *i.e.* group learning without punishment.

is 1.68. Number of mistakes on each round is plotted against successive rounds in Fig. 5. Even though neither group were punished on the non-shock side, Group I was more accurate on account of the shocks given

for errors on the shock sides. The shocks evidently brought about generalized motor response of averting contacts on both sides of the track.

In the case of errors the reliability coefficients were calculated from the averaged even-odd pairs of scores for the 20 trials. On the non-shock sides it is 0.896 $\pm$ .029 for Group I, and 0.857 $\pm$ .032 for Group II; similarly, on the shock sides they are 0.736 $\pm$ .069 and 0.766 $\pm$ .052.

Errors of the non-shock part by the Experimental Group are to be accounted for partly by transfer. The shock spurred the *S*s on to an early generalization of avoiding painful annoyances. They found apparent forward movements actually to be backward actions, and conversely; and also that seeming right- and left-hand directions were as they appeared. Some were unable to report on any learning cues; they stated that they learned to follow the groove in an unanalyzed way. The accuracy on one side of the star pathway resulting from punishment for errors on the other side, points to transfer as a plausible partial explanation.

Also, intentional inhibition plays a part in building up new sensorimotor coördinations. Mistakes leading to pain soon urge the *S* to anticipate the consequences of touching the shock sides. Mere starts follow in various directions until the successful direction is hit upon, thus mistakes may be entirely eliminated—naturally it takes more than average time. Thus learning would seem to be going forward on an incipient motor level.

Muscular tensions is a third possible factor. They were reported as being present in different degrees by all the *S*s of Group I. They seemed to be noticed principally in the fingers, arms, legs, face and neck. Some punished *S*s showed jerky responses during the first few rounds of the star.

Conclusions

(1) Mild electrical shock as punishment for errors in the case of mirror star-tracing, increased the time required for learning.

(2) On a given number of circuits in the mirrored star, punishment serves to decrease the number of errors.

(3) The effect of punishment is more pronounced on the later than on the earlier stages of star-tracing.

(4) Electrical shock applied as punishment for errors on one side of a course reduces the errors on the other side, although no shock is given from that side.

MODULATION OF THE OPTIC NERVE-CURRENT AS A BASIS FOR COLOR-VISION

By GLENN A. FRY, Washington University

Troland originated the idea of the modulation of the current of impulses in a single optic nerve-fiber as a basis for color-vision.[1] He proposed that the impulses passing along a given fiber could "be spaced in different characteristic ways to represent the various colors, luminosity still depending upon the total number of pulses per unit time."[2] One kind of modulation could consist in a periodic variation in the frequency of impulses as indicated in Fig. 1. The waves in the solid line represent impulses succeeding each other. These impulses must be conceived to be fundamentally the same in accordance with the all-or-none principle of nerve conduction. The frequency is high at A, low at B, high again at A′, and low again at B′. The dotted line represents this periodic variation in frequency.

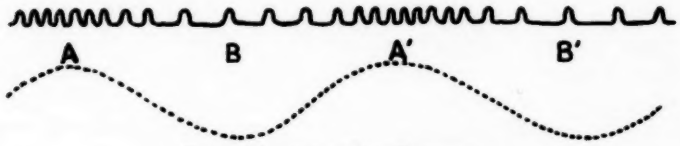

FIG. 1. MODULATION OF FREQUENCY

According to this scheme there are four independent variables: (1) the average frequency of impulses; (2) the frequency; (3) the form; and (4) the amplitude of the modulation waves. Intensity might depend upon the average frequency of impulses, and hue might depend upon either the form or the frequency of the modulation waves or both.[3] An unmodulated current of impulses might correspond to a sensation of white, and saturation of hue might depend upon the amplitude of the modulation waves. Blackness might depend upon the degree of inactivity.

In the case of a flickering color, flicker as well as hue must depend upon some variation in the sequence of impulses. It might be supposed that for a non-flickering color the periodic variation in frequency which subserves hue would be smoothly graded, and that for a flickering color the sequence of impulses would be irregular; however, this irregularity would not need to interfere with the slow modulation rhythms subserving hue.

The modulation theory relates strictly to the transmission of color-impressions from the retina to the cortex, and no assumption need be made as to what happens after the cortex is reached.

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* Accepted for publication May 5, 1933.

[1] The present paper is published from the Department of Ophthalmology, Oscar Johnson Institute, Washington University, St. Louis.

[2] L. T. Troland, The enigma of color vision, *Amer. J. Physiol. Optics*, 2, 1921, 23-48, esp. 43.

[3] Hue might depend upon frequency only indirectly as varying the frequency would change the form of the waves; or the cortical processes subserving color-vision might be affected separately by both frequency and form.

If color-vision depends upon the modulation of the optic nerve current, the problem arises as to how steady stimulation with colored light modulates the optic nerve current, for most of the color-sensations which come to us in everyday life are produced in this way. This is a problem of fundamental importance, but no data are at hand which throw any light upon it. Troland himself pointed out that when we raise the question as to the retinal mechanism by means of which colored light modulates the optic nerve current we find ourselves at a standstill.[4]

Although the principle of modulation can be applied to a single nerve-fiber, it is quite possible that a single end-organ is never stimulated alone, and quite often a large area of the retina is stimulated and a large group of fibers simultaneously brought into activity. Can these fibers function independently of each other as isolated units, or must the modulation rhythms in the various fibers be synchronized? Since neighboring retino-cortical paths interact with each other,[5] the modulation rhythm in one path might interfere with the rhythms in neighboring paths unless the rhythms in the various paths were synchronized. There is no final proof, however, that interaction would interfere.

The modulation theory requires the assumption that modulation patterns can be transferred across synapses without being interfered with in the process. An experiment of Adrian and Matthews[6] bears upon this problem. They exposed the retina of a conger eel to an intermittent white light and found intermittent phases of activity in the optic nerve fibers. The rate of these phases corresponded to the rate of stimulation as long as the rate of stimulation did not exceed 13 per sec., and this correspondence occurred in spite of the fact that synapses intervened between the retinal end-organs and the optic nerve-fibers. It is plausible to assume that during each phase of activity each fiber responded a number of times in rapid succession, since a nerve-fiber is capable of responding 200 or more times per sec. In other words, we have here an experimental demonstration that intermittent stimulation of the retina may set up modulated currents of impulses in the optic nerve fibers.

If intermittent stimulation of the retina can set up a modulated current of impulses in the case of man as well as in the case of the conger eel, the color-sensations produced with intermittent white light might be attributed to this modulation. With this in mind let us consider the experimental facts described in the remainder of this paper.

The disk illustrated in Fig. 2 is made of cardboard. It is 10 in. in diam. and the black band is 1 in. wide. B is an open slit. This disk was rotated before a bright surface whose position and size with respect to the disk are indicated by

[4] Troland, *op. cit.*, 43.

[5] For a summary of the evidence that the retino-cortical paths interact with each other, the reader is referred to F. B. Boswell, Visual irradiation, *Harvard Psychol. Stud.*, 2, 1906, 75-107. R. S. Creed (The physiological integration of sensory processes within the grey matter of the central nervous system: A critical review, *Brain*, 54, 1931, 29-53) has given a critical and historical survey of the evidence. In a series of investigations with the retina of the conger eel E. D. Adrian and R. Matthews (The action of light on the eye, *J. Physiol.*, 63, 1927, 378-414; 64, 1927-28, 279-301; 65, 1928, 273-298) have furnished abundant evidence that the retino-cortical paths interact with each other at the synapses of the retina.

[6] Adrian and Matthews, *op. cit.*

the dotted line. This bright surface consisted of a ground glass illuminated from behind with two 250-watt lamps. The front of the disk was illuminated with a 250-watt lamp placed 12 ft. away and at an angle of 60°. The situation was such that the brightness of B, the angular size of B, and the speed of rotation could be varied independently of each other, while the brightness of A and A′ and the brightness of C were held constant. When the disk is rotated in front of the bright surface, that part of the bright surface corresponding to the path of the moving slit intermittently stimulates the retina and produces a flickering color which changes hue whenever either the brightness of B, the angular size of B, or the speed of rotation is varied. The observations were made at a distance of 12 ft.

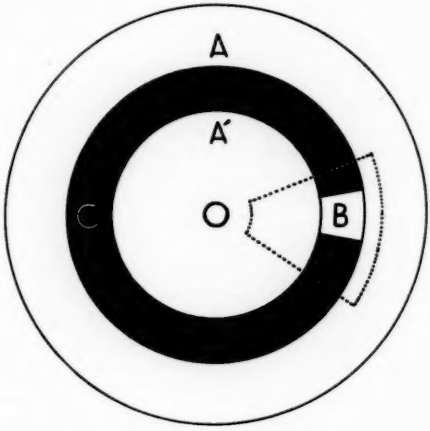

FIG. 2. STIMULUS DISK
B is an open sector, and the dotted lines show a sector of bright illumination behind the disk

The data in Table I show the effect of increasing the frequency of stimulation in several instances in which the duration of each stimulation is held constant. The brightness of B (402.7 c. per sq. ft.), the brightness of A and A′ (0.506 c. per sq. ft.), and the brightness of C (0.014 c. per sq. ft.) are all kept constant.

As the frequency is increased, the color changes, in the case of the 0.46σ stimulus, from blue to green and finally to white. In the case of the 1.39σ stimulus the color changes from purple to blue but then turns white without changing further in hue. In the case of the 2.78σ stimulus, the purple changes to white without changing in hue at all. In the case of the 6.94σ stimulus, the color changes from purple to purple-pink, to pink, and finally to white. In other words, purple and the 2.78σ stimulus seem to be transition points; with a shorter stimulus the color changes from purple toward green; with a longer stimulus the color changes in the opposite direction, *i.e.* from purple toward pink; but with the 2.78σ stimulus the color does not change in either direction.

Varying the frequency of stimulation changes the *frequency* of the modulation waves (at least it so happens in the case of the conger eel) and perhaps also the *form* of the waves. If color-vision depended on modulation, then the change of hue accompanying the change in frequency of stimulation could be attributed to either of these factors.

TABLE I

SHOWING THE EFFECT OF INCREASING THE FREQUENCY OF STIMULATION

Frequency of Stimulation	Color for Different Durations of Stimulation			
	0.46σ	1.39σ	2.78σ	6.94σ
4 per sec.	Blue	Purple	Purple	Purple
6 per sec.	Blue	Violet	Purple	Pink-purple
8 per sec.	Greenish blue	Violet	Light purple	Pink(blue borders)
10 per sec.	Greenish white	Light blue	White	White
12 per sec.	Greenish white	White	White	White
15 per sec.	White	White	White	White

The data in Table II show the effect of varying the angular size of B and the brightness of B, when the speed of rotation (8 per sec.), the brightness of A and A′ (0.506 c. per sq. ft.), and the brightness of C (0.014 c. per sq. ft.) are all kept constant.

TABLE II

SHOWING THE EFFECT OF VARYING ANGULAR SIZE AND BRIGHTNESS

Size of Slit	Duration of Stimulation	Color for Different Brightnesses of B			
		402.7 c. per sq. ft.	219.3 c. per sq. ft.	56.4 c. per sq. ft.	6.4 c. per sq. ft.
1°	$.35\sigma$	Blue	Bluish green	Dark gray	Dark gray
5°	1.74	Purple	Blue	Greenish blue	Dark gray
10°	3.74	Purplish white	Bluish white	Blue	Dark gray
15°	5.21	Pink-purple	Purplish white	Blue	Dark gray
20°	6.94	Pink (blue borders)	Purplish white	Bluish white	Dark gray
30°	10.41	Pink (blue borders)	Purplish white	Purplish white	Bluish green
60°	20.82	White	Pinkish white (blue borders)	White	Greenish blue
90°	31.23	White	Pinkish white (blue borders)	White	Greenish blue
120°	41.64	White	White	White	White

For a given duration of stimulation increasing the intensity causes the color to change progressively in the direction from green toward pink. Below a certain level, however, decreasing the brightness of B causes the color to change to gray.

For a given intensity of stimulation increasing the duration causes the color to change progressively in the direction from green toward pink. In other words increasing the duration of stimulation is equivalent to increasing the intensity. Beyond a certain limit, however, increasing the duration causes the color to change to

white. This loss of hue is due, perhaps, not to the increase in duration of stimulation, but to the decrease of the interval between stimulations, for the size of C decreases as the size of B increases.

If it assumed that the *frequency* of the modulation waves is kept constant by keeping the frequency of stimulation constant, the change of color accompanying a change in duration or intensity of stimulation must be attributed to a change in the *form* of the waves, *i.e.* if the facts are to be explained on the modulation theory.

So far we have dealt with the rôles played by the speed of rotation, the angular size of B, and the brightness of B. Now what is the rôle played by the brightness of A and A′? In Table III the results obtained when A and A′ are brighter than C are compared with the results obtained when A and A′ are no brighter than C, the brightness of B (402.7 c. per sq. ft.), the brightness of C (0.014 c. per sq. ft.), and

TABLE III

SHOWING THE EFFECT OF VARYING THE BRIGHTNESS OF THE SURROUNDINGS

Size of Slit	Apparent Color of the Band	
	Brightness of A and A′ 0.506 c. per sq. ft. Brightness of the Black Band 0.014 c. per sq. ft.	Brightness of A and A′ 0.014 c. per sq. ft. Brightness of the Black Band 0.014 c. per sq. ft.
1°	Blue	White
2°	Violet	White
5°	Purple	White
10°	Purple	White
15°	Pink-purple	White
20°	Pink (blue borders)	Pink (blue borders)

the speed of rotation (8 per sec.) being held constant. As can be seen in the table, pink can be produced without the aid of bright surroundings, whereas purple, blue, and green cannot. How do bright surroundings aid in the production of these colors? It might be supposed that bright surroundings suppress the retinal activity during the intervals between stimulation and thus help to modulate it.

Summary. Troland's theory that the transmission of color impressions from the retina to the cortex depends upon the modulation of the optic nerve-current shows itself to be compatible with modern theories of nerve-conduction. An experimental basis for the theory is provided by the production of color-sensations with intermittent white light, inasmuch as it may be assumed that intermittent stimulation modulates the optic nerve-current. The dependency of these color-sensations upon frequency, duration and intensity of stimulation, and upon the brightness of surroundings, can be adequately accounted for on the modulation theory.

THE EFFECT OF SERIAL POSITION IN THE CONTINUOUS MEMORIZATION OF NUMBERS

By MILDRED B. MITCHELL, George School, Pa.

Robinson and Brown investigated the effect of serial position upon memorization when there was an interval of 10 sec. between the various trials in learning a series of numbers and nonsense syllables.[1] The present report deals with lists of 10 three-place numbers learned on McGeogh's memory drum by the *anticipation* method with *no* extra time interval between the various trials. The last number thus became the cue for the first and there was no break in the 2-sec. exposures until the list had been given correctly once from the first through the tenth number.[2] This last requirement of giving the numbers from 1 through 10, instead of stopping *S* whenever he responded correctly to any 10 consecutive numbers, may have tended to give results more like those of Robinson and Brown than would otherwise have been the case.

Out of over 23,000 correct responses given by 11 *S*s during the learning of 552 lists, there were more of these given for the first number in the list than for any other number in the list. The second number ranked second in quantity of correct responses, the third number ranked third, the last number ranked fourth, while the fourth number ranked a close fifth. Table I gives the total number of correct responses for each position in the series, the percentage of the total correct responses given in each position, and the ranked order for the 10 positions. As may be seen from this table, the other 5 positions vary only slightly; in fact, there is a range of less than 2% between number 8 which ranks last and number 5 which ranks sixth.

These results are in essential agreement with those of Robinson and Brown. Both primacy and finality facilitate learning, but the primacy effect is greater and extends farther than the finality effect.

The effect of the position of a number upon its ease of being learned is also shown by studying the correct responses in learning a particular list. For instance, list 33 was learned by all 11 *S*s. The number of correct responses for each number in the list was tabulated. The number of trials needed to learn the list to one perfect repetition ranged from 6 to 22. Since the *S*s did not attempt to give the numbers after they had made one perfect repetition, each number was tabulated as if correct from the perfect repetition through 22 trials. A few errors, no doubt, would have occurred if further trials up to 22 had been given to all the *S*s. Had the experiment been set up for the purpose of studying this problem, a sufficient number of trials would have been given to have eliminated the necessity of this inaccuracy. Since the same procedure has, however, been applied to all the numbers in the list, they

* Accepted for publication April 14, 1932. From the Psychological Laboratory of Yale University. The writer is indebted to Professor E. S. Robinson under whose direction this experiment was performed.

[1] E. S. Robinson and M. A. Brown, Effect of serial position upon memorization, this JOURNAL, 37, 1926, 538-552.

[2] A more detailed account of the method and procedure is given in the study of which this was a part. Errors in the memorization of numbers, this JOURNAL, 45, 1933, 1-16.

may be considered comparable. Trial 1, the first presentation, was always zero. The first number in the list was given correctly by all 11 *Ss* after the fifth trial with 3 exceptions when it was missed by only 1 *S*. The learning curve for this number (Fig. 1), therefore, shows a rapid rise and is negatively accelerated.

TABLE I

NUMBER CORRECT RESPONSES BY POSITION IN LIST
(11 Ss learned 552 lists)

	Serial Position									
	1	2	3	4	5	6	7	8	9	10
No.	3447	3253	2932	2348	2023	1833	1944	1570	1717	2371
%	14.7	13.9	12.1	10.0	8.6	7.8	8.3	6.7	7.3	10.1
Rank	1	2	3	5	6	8	7	10	9	4

The sixth number, which ranks lowest for this particular list (it has 127 correct responses as compared with 130 for number 8), has a learning curve which is positively accelerated at the beginning (Fig. 1). The last number has a learning curve similar to that of the first number, while the eighth number has a learning curve more like that of the sixth number. Hence the more favorable positions have rather sharply decelerated curves, while there is some evidence for positive acceleration in the curves for the unfavorably situated numbers. This also is consistent with the results obtained by Robinson and Brown.

Conclusion. In conclusion, it may be stated that omitting a time interval between the trials in memorizing lists of numbers under the conditions of this experiment does not materially change the effect of the serial position. The first few and the last numbers still maintain an advantageous position with a negatively accelerated learning curve, while the others tend to be learned with greater difficulty and to have a positively accelerated learning curve.

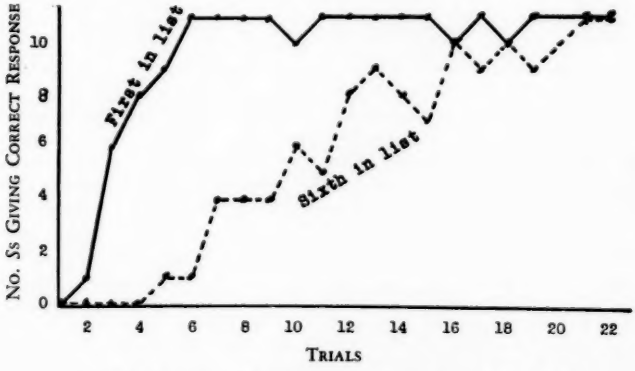

FIG. 1. LEARNING CURVES FOR POSITIONS 1 AND 6 IN A LIST LEARNED BY 11 *Ss*

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

VII. The Affective Value of Combinations of Odors

By Willard Spence and J. P. Guilford

In a previous study,[1] the senior author proposed the possibility of predicting the affective value of a combination of stimuli from the affective values of its two components. Quantitative laws were suggested to apply to combinations of colors. The following theoretical conclusion was reached concerning the summability of affect: "There are some indications that the summation of feeling is an additive one and not a product, and that the combined value is not an algebraic sum but a weighted mean of P and U or of some simple function of P and U."[2] By 'P' was meant the more pleasing member of the combination, and by 'U' the less pleasing member. Quoting further, "It is suggested that there is a limit to the prediction of the 'affective whole' from its 'components,' and that the emergent or 'Gestalt-effect' may be measured by the index of non-determination, K^2."

It is the purpose of this study to test the validity of these conclusions by using combinations of odors. There are several reasons why combinations of odors might be better material with which to test these assertions than are combinations of colors. The latter must necessarily be separated spatially; the former can be more intimately mingled and fused. This fact might, on the one hand, produce a more unitary perceptual experience and therefore give a more complete amalgamation of affective responses. One cannot be sure that two colors, although juxtaposed in space, will be taken by his *Os* as a unitary perceptual experience. On the other hand, because the odors become more unitary in their combinations, new and emergent perceptual properties might give rise to unpredictable affective reactions. Those who have had much to do with the creation of perfumes know that this may be true. Our mode of presentation of the odor stimuli did not permit complete mixtures of stimuli such as we find in perfumes, however. We presented one odor to each nostril, so that any resulting mixture had to be a cortical one as in the usual psychological or physiological study of mixtures of odors. To allow the vaporous stimuli to mingle before striking the sensitive surfaces of the olfactory organs leaves us in doubt as to just what kind of a stimulus we are applying, *i.e.* as to just how much chemical action might have occurred before the vapor reaches its goal. Our combinations of odor were perhaps not so different from combinations of color after all. There was a spatial separation of the two components, except that olfactory localization is much inferior to visual localization, and in addition to this there was the opportunity for cortical blending. This type of blending was of course not possible in the case of the combinations of color.

The odors that were used in the combinations were selected after some preliminary

* Accepted for publication March 16, 1932.

[1] J. P. Guilford, The prediction of affective values, this Journal, 43, 1931, 469-478.

[2] *Op. cit.*, 478.

trials. It was desired to select single odors that extended over the whole affective scale at approximately equal intervals. Eight *O*s, including 5 men and 3 women, were used throughout the experiments. Two were graduate students in psychology, 1 an instructor, and the remaining 5 were undergraduates. For the type of observations demanded, we could see no difference between the practiced and the unpracticed *O*s. The *O*s first gave judgments, using a scale of values, upon 24 single odors. After two ratings by each *O*, we pooled the 16 judgments upon each odor and thus found its approximate affective value on our scale. From the 24 odors we then selected two groups, one of 6 and one of 7 odors, which extended evenly over the whole range of affective values found for the 24 odors. Each one of the 6 odors was combined with each one of the 7 in order to make the 42 combinations which were used in this study.

The experimental procedure with the single odors was as follows: A double olfactometer was set up in the opening of a large screen which separated *O* from *E*. Two bent glass tubes were inserted in the olfactometer. *O* adjusted the ends of the tubes in his two nostrils. *E* uncorked a bottle containing an odorous substance and inserted the other ends of the glass tubes into it. At a signal from *E*, *O* executed a long slow inhalation, and held his breath until a judgment was reached. This was found to be necessary, for in the very first experiments it was found that *O*'s judgment might change during an exhalation because the quality of the odor itself seemed to change. *O* had the following instructions before him in typed form:

After you are adjusted to the olfactometer, let *E* know by giving a signal. When *E* says 'Ready' he is preparing the stimulus. When he says 'Now' you are to inhale with one long slow inhalation. Keep your inhalations constant from one experiment to another in speed and in length, and hold your breath until your judgment is made. You are to judge the pleasantness or unpleasantness of the odor on the 9-point scale given below. If you cannot make a judgment, you may ask *E* to repeat the experiment. You will be allowed not more than two trials. Scale of values: (9) greatest possible pleasantness, (8) extremely pleasant, (7) moderately pleasant, (6) mildly pleasant, (5) indifferent, neither P nor U, (4) mildly unpleasant, (3) moderately unpleasant, (2) extremely unpleasant, (1) greatest possible unpleasantness."

This form of scale was adopted for two reasons. Conklin has found that with unpracticed *O*s a 9-point double scale is best.[3] We did not make the unpleasant steps negative and the pleasant ones positive because Young has shown how this breaks the continuum into two parts and produces a bimodal distribution of judgments.[4] The more accurate methods, of paired comparisons and of ranking, are out of the question in the judgment of odors for very obvious reasons. The use of the rating scale method in this connection has given rather good results for a number of investigators in the past.[5] We have not taken the trouble to test the reliability of the measurements which were used in this study. Inspection of the same *O*'s repeated judgments upon the same single odor or combination of odors shows extremely few deviations of more than one point on the 9-point scale. Furthermore, the values

[3] E. S. Conklin, The scale of values method for studies in genetic psychology, *Univ. of Ore. Stud.*, 2, 1923, (no. 1), 1-36.

[4] P. T. Young, Studies in affective psychology, this JOURNAL, 42, 1930, 17-27.

[5] See esp. J. H. Kenneth, A few odor preferences and their constancy, *J. Exper. Psychol.*, 11, 1928, 56-61.

assigned by different *O*s to the same odor show surprisingly few deviations greater than one point. By pooling the judgments of 8 *O*s, individual peculiarities of preference are cancelled out and something approaching an 'objective' measure of affective value of the stimuli is reached. In order to obtain the best measure of 'objective' affective value, one should follow the technique of Beebe-Center;[6] *i.e.* find the extent to which each *O*'s judgments are correlated with the objective values and weight them accordingly. Our pools of judgments may be regarded as unweighted pooled observations of the objective affective values.

The procedure in judging the combinations is best described by quoting the instructions that were given to the *O*s.

The experiments will be conducted just as before. When *E* says 'Now' you are to inhale deeply and slowly and at a uniform rate, and to make a judgment before you exhale. Try to give your attention to the whole odor-combination. Try to take it as a single total sensory experience. If you should fail to do this, be sure to tell *E* what kind of a combination you experienced and what features of it determined your judgment. Add any other comments that you think are significant.

The 13 odors that were chosen for combination will be found listed in Table I. Their mean affective values are given as are the mean affective values of the 42 combinations. In the final results we have shifted the zero point of the scale to the indifference point so it may be seen at a glance whether a particular affective value is P or U.

The order of presentation of the combinations was counterbalanced so that no odor was given twice in succession, and so that the same odor when it was repeated in the same hour would be presented to the two nostrils alternately. Each *O* judged every combination twice, on two different days. The order of presentation on the second day was just the reverse of that on the first day, so that each pair of odors could be reversed in arrangement in the two nostrils. After all the judgments of combinations had been completed, the 13 single odors, again mingled with the entire list of 24, were judged two more times by every *O*. There were, therefore, 32 judgments of each single odor and 16 of each combination. In both cases the glass tubes of the olfactometer were changed after every judgment. After every experimental hour they were deodorized and sterilized by boiling for 3 min. in water and drying under air currents. *O* always rested for about 2 min. after making a judgment. A large electric fan kept the air of the room continually stirred up and kept up a circulation of fresh air entering the room.

Table I shows that there is a close agreement between the values of the single odors and those of the combinations. The 6 odors at the top and the 7 at the side are arranged in descending order, and it will be noted that the trend of the whole table is in the same direction. This is perhaps not very surprising. But can we formulate any mathematical equations by which one can predict the value of a combination from the values of its two components? And what form will these functions take? In the case of color combinations, previously cited, the best laws of combination were probably linear, although due to the small number of measurements and to their peculiar distribution there was some uncertainty about this point.[7] In the case now under consideration, the component stimuli were especially chosen

[6] J. G. Beebe-Center, General affective value, *Psychol. Rev.*, 36, 1929, 472-480.
[7] Guilford, *op. cit.*, 472-475.

for the occasion, with affective values ranging widely over the whole scale and evenly distributed. The number of combinations is also relatively large and we should be able to come to a more definite conclusion concerning the law of summation of affect.

We first examined the values of combinations with each of the 13 single odors in turn. For each odor at the top of the table there are 7 values of combinations. These may be taken as functions of the 7 values of the single odors at the left hand side of the table. Likewise, for every odor at the left there are 6 values of combinations with the 6 odors at the top of the table. When these 13 sets of paired values are plotted, they are all obviously linear, and when we remember that they extend over a wide range of the scale we may assume that they would be linear throughout. The slopes of the lines, however, are not exactly the same. They range from 0.21

TABLE I

AFFECTIVE VALUES OF THE 13 ODORS AND OF THE 42 COMBINATIONS

(b = slope of line; a = y-intercept, *i.e.* indifference value; and r = coefficient of correlation)

	*	pepper-mint	cinna-mon	musk	ben-zene	creo-sote	stale cheese	b	a	r
	*	2.31	1.19	.69	−.56	−1.53	−2.19			
orange	1.84	2.19	2.00	1.43	.00	−.06	.44	.56	1.01	.93
anise	1.78	1.75	1.75	1.37	.12	−.38	.56	.47	.87	.86
almond	.97	1.81	1.12	.62	−1.06	−.94	−.63	.69	.16	.93
bergamot	.59	1.00	.93	.37	−.19	−1.19	−1.07	.58	.00	1.00
benzoin	−.22	1.00	.31	.25	−1.38	−2.00	−1.88	.79	−.60	.93
amber	−1.06	1.25	.25	−.88	−.94	−1.07	−1.82	.63	−.53	.96
carbon bisul-phide	−3.34	−2.50	−2.94	−3.37	−3.13	−3.16	−3.69	.21	−3.13	.85
b		.78	.88	.90	.56	.52	.79			
a		.87	.42	−.10	−.98	−1.32	−1.22			
r		.91	.97	.99	.92	.75	.98			

* In this line and in this column are given the affective values of the single odors.

when carbon bisulphide is the standard or constant member of the combinations to 0.90 when musk is the constant one. They are all less than 1.0, which indicates that the total range of values for the combinations with any constant odor is never as great as the range of the variables that are combined with it. Since carbon bisulphide was the most unpleasant odor of the lot and gave the smallest slope, and since musk was among the pleasant group, it occurred to us that the slope of the line might be proportional to the value of the constant member of the combinations. A correlation between the two things, slope of line ("b" in the table) and the affective value of the single odors was only 0.30. This insignificant relationship was almost entirely due to the very small slope in the case of carbon bisulphide alone, and we shall point out later that this is probably due to the fact that the carbon bisulphide tended to dominate all the combinations in which it appeared. The effects of dominance and fusion will be discussed later.

Another item of interest in Table I lies in what we have called the 'indifference values' of the single odors. These are given in the column and the line marked "a" in the table. They are merely the y-intercepts of the 13 linear functions. We have called them 'indifference values' because they are the values of the combina-

tions for given standard odors when the other odor lies at the indifference point. We had no single odors with that exact value, of course, but from the linear functions we can predict what the effect of such odors would be. One striking thing about these indifference values is that with one exception, and that is in the case of benzoin, they are all nearer the zero point on the scale of values than the standard odor itself. That is, the combination of any odor, whether P or U, with a second indifferent one is always less P or U, as the case may be, than that odor is alone. This may be due to the manner of presentation of the stimuli. It may be more pleasant or unpleasant to have the same P or U odor in both nostrils than to have it in only one with an indifferent one in the other nostril. This fact may also be a cause of the slopes all being less than 1.0. Or, both of these facts may be due to one of our former conclusions, that the combined affect is not an algebraic sum but a weighted mean of its two components.

One fact that might be of great practical interest, is that these indifference values are almost perfectly correlated with the scale values of the standard odors. The coefficient is 0.96. Thus, by merely knowing the affective value of any given odor, one could predict very accurately its indifference value or the value of any indifferent odor combined with it. Now if one could also predict in a similar way the slope of the 'function of combination' for the same odor, one would have all the necessary information for the prediction of the combined affect from its components. By 'function of combination' is meant the linear equation for any given standard odor, with the value of the combination as the dependent variable and that of the second odor combined with the standard as the independent variable. Unfortunately, there is no way of predicting the slope of the line from the value of the standard odor, as we have shown in a preceding paragraph.

The last row and column of Table I give the coefficients of correlation between the variables in the different functions of combination. There is little of interest in them, except that they are all high enough to justify our acceptance of the linear relationships. Three of them are conspicuous for being below 0.90; these are for creosote, anise, and for carbon bisulphide. No reason is suggested for this, and in view of the very small number of measurements, 6 or 7 for each function, it is hardly worth one's while to look for any reason.

We began by raising the question as to whether the law of combination found for color stimuli would also hold for odor stimuli. This law of combination distinguishes between the more pleasing component, P, and the less pleasing component, U. It was admitted that the distinction might be somewhat artificial, but it seemed to be the only one that could be made. If we apply the same assumption to our data with odors, we find the law fully verified. In the previous paragraphs we have found that when one member of a number of combinations is held constant, the value of any combination is a linear function of the variable members. Therefore we need only consider first powers of P and U in using least square treatments. The necessary constants were computed by the method of least squares with the three variables, C (the affective value of the combination), P, and U. The following equation resulted: $C = .54\,P + .69\,U - .21$. The coefficient of multiple correlation between the predictions from this equation and the observed affective values is 0.967. The standard error of the estimate is 0.39. Both these last two values are uncorrected for the number of variables and for the number of observations. The

number of variables is so small and the number of observations is so large that the correction would be very small.[8]

This result can be regarded as a complete vindication of the assumption of this type of combinational law for the summation of affects. This is all the more significant as a law of affect, in view of the fact that it holds for stimuli in two sense departments, visual and olfactory, and also the fact that the types of combination, perceptually or physiologically regarded, are not quite the same in the two cases. Some concern was expressed at the beginning of this report about the fact that combinations of odors are phenomenally different from combinations of colors. It was suggested that the combinations of odors are more unitary, due to the fact that the two components are less separated spatially. This may account for the result that the multiple correlation coefficient is slightly higher here in the case of odors than it was in the case of colors.[9] If R^2 measured the percentage of total determination of the combined affect by its components, that determination amounts to about 94% in the case of odors. The remaining 6% might be due to errors of measurement, but we believe that we have found another factor to account for at least a part of it. That factor is the lack of fusion of the odors, or the dominance of one odor by another.

In the previous article it was suggested that this remaining percentage, (K^2), could be used to indicate the 'Gestalt effect' or the emergent properties arising from the combination. In the case of colors, with women *O*s especially, this remainder was reasonably large. Certainly, if the law of combination was correct, there were other unknown factors at work, and these factors might be attributable to the totality of the experience and not to the components themselves. In the case of odors, however, very little remains to be accounted for. This would mean that if there is a 'Gestalt effect' it must have little potency. But the factor of dominance, which we think is the most important remaining factor, is by its very nature a Gestalt effect. There could be no dominance if there were not a relationship between two odors. It might be logically argued, of course, that the property of dominance exists in the dominant odor and the property of recessiveness exists in the odor that is dominated. Carbon bisulphide, for example, tended to dominate everything with which it was paired. Cinnamon tended to dominate nothing else and was dominated by many of those with which it was paired. On the other hand, a clear order of combination is not easy to make out. It does not always follow that an odor *A* that dominates odor *B* which in turn dominates odor *C*, will also dominate odor *C*. We make these statements with some hesitation, because our *O*s were untrained in the observation of odors, and because the number of reports of actual dominance are limited.

In the instructions for judging the combinations, *O* was told always to render a judgment as to whether the two odors fused well, and if they did not, to state which one of the pair was dominant. Out of a total of 672 judgments, only 182 were reported to be cases of dominance; the remaining 490 pairs of odors were fusions. The number of cases of dominance is too small to give any adequate idea of the relative dominance of the various odors. We were able to select 17 specific combinations of odors, however, in which it was reasonably apparent that one odor

[8] M. Ezekiel, *Methods of Correlation Analysis*, 1930, 121.
[9] Guilford, *op. cit.*, 474.

of the pair tended to dominate over the other one. Each pair had been judged only 16 times. In the 17 cases of which we speak, the one odor dominated over the other one from 4 to 10 times out of a possible 16. This gave us only a rough criterion for distinguishing between the dominant and the recessive member of a combination so that we might find an empirical weighting for the two in the prediction of the value of the combination.

Applying least square procedures again, this time using the dominant member (D) and the recessive member (R) as the two independent variables, we found a regression equation for the prediction of C, the value of the combination. The equation is as follows: $C = .85\ D + .21\ R - .40$. The coefficient of multiple correlation, uncorrected, is 0.947, and the standard error of estimate 0.51. When the dominant and recessive members of combinations are weighted as in the above formula, two thirds of the predictions will fall within 0.51 of the observed values. It is significant that the dominant member is weighted about four times as much as the recessive one. If this factor of dominance were taken into account in addition to the distinction between the P and the U components, predictions of combined affects from odors might be made almost perfectly, within the margin of errors of measurement. And if the degree of dominance were considered, the predictions would probably be even more accurate. For example, carbon bisulphide, which dominated almost every combination in which it appeared, was almost the sole determiner of the values of those combinations.

Summary and Conclusions

This study was undertaken in order to test a law of affective summation previously proposed for the affective values of combinations of color. Two groups of single odors, 13 in all, were selected from a list of 24 odors because they ranged evenly over a wide extent of the pleasant-unpleasant continuum. The affective values of the 13 single odors and of 42 combinations of them were determined by means of rating scale judgments. The following conclusions were reached after a functional analysis of the results.

(1) For any given odor, the affective value of the combination of a second odor with it is a linear function of the affective value of the second odor. This holds true without regard to the degree of fusion of the two odors.

(2) The affective value of a combination of odors can be predicted rather accurately from the affective values of its two components, the more pleasing and the less pleasing component.

(3) Again, as in the case of combinations of color, the affective reaction to a combination of odors is apparently not an algebraic sum of the affects belonging to the single odors, but it is rather a weighted mean of the two.

(4) When one odor of a pair is dominant, it carries much more weight in determining the affective value of the combination than does the recessive odor. The relative weight of the dominant odor probably depends upon the degree of dominance.

APPARATUS

THE HATHAWAY GALVANIC APPARATUS AS AN INSTRUMENT OF RESEARCH

By T. W. Forbes, Columbia University

Since the advent of Hathaway's "psychogalvanometer" a number of investigators have made use of it for research. Among these are Hathaway,[1] Porter,[2] Porter and Copeland,[3] Ruckmick,[4] Smith,[5] Crosland,[6] Dysinger,[7] and Misbach.[8]

As a means of demonstrating the galvanic phenomenon the apparatus has proved its worth, since the reaction is quickly, easily, and directly exhibited. For research purposes, however, the fact that a galvanic response is indicated is not sufficient. Where statistical comparison is made of average deflections called out by certain types of stimuli, it is especially important that the deflections be accurately proportional to actual galvanic responses. If the apparatus introduces any distortion, this distortion must be known and corrected for before statistical treatment of deflection scores will yield valid data concerning galvanic reactions. Davis has questioned whether the proportionality of deflection to reflex remains constant in the Hathaway apparatus.[9] Our own experience led us to question whether the current through the subject used by the Hathaway apparatus could be sufficiently well standardized. It seemed, for these reasons, that it would be worth while to determine the actual physical characteristics of the apparatus.

Electrical and physical characteristics. In determining these characteristics we substituted in place of the human *S* the simplest possible combination of physical resistances and capacities which could validly be used. This combination consisted of a variable resistance and a variable capacitance connected in parallel, as shown in Fig. 1. This figure also shows the Hathaway galvanic circuit in simplified form in order to indicate the exact relationship of *S*'s circuit to that of the apparatus. It has previously been shown by Philippson,[10] Cole,[11] McClendon,[12] and Strohl,[13]

[1] S. W. Hathaway, A comparative study of psychogalvanic and association time measures: A new psychogalvanic apparatus, *J. Appl. Psychol.*, 13, 1929, 632-646.

[2] J. P. Porter, The psychogalvanic compared with other measures of emotional reactions, *Psychol. Bull.*, 26, 1929, 156-157.

[3] J. P. Porter and H. A. Copeland, Association and psychogalvanic responses during early adolescence, *Proc. IX Int. Cong. Psychol.*, 1929, 349-350.

[4] C. A. Ruckmick, Emotions in terms of the galvanometric technique, *Brit. J. Psychol.*, 21, 1930, 149-159.

[5] F. O. Smith, Variations in the galvanic response, *Psychol. Monog.*, 41, 1931, (no. 187), 142-152.

[6] H. R. Crosland, Objective measurements of emotion, *Univ. Oregon Pub.*, 1, 1931, (no. 3), 153-196.

[7] D. W. Dysinger, A comparative study of affective responses by means of the impressive and expressive methods, *Psychol Monog.*, 41, 1931, (no. 187), 14-31.

[8] L. E. Misbach, Effect of pitch of tone-stimuli upon body resistance and cardiovascular phenomena, *J. Exper. Psychol.*, 15, 1932, 167-183.

[9] R. C. Davis, Factors affecting the galvanic reflex, *Arch. Psychol.*, 18, 1930, (no. 115), 12-13.

[10] M. Philippson, Les lois de la resistance électriques des tissues vivants, *Bull. (Sc.) Acad. roy. belgique*, 7, 1921, 387-403.

that this combination is too simple to represent the electrical characteristics of human skin accurately. It does represent, however, a rough approximation.

Having set up our circuit as shown in Fig. 1, we adjusted the Hathaway apparatus according to the mimeographed directions accompanying the apparatus, for each of a series of values of resistance (R) and capacitance (C). According to the directions this setting should standardize the current through the external circuit which includes the resistance and capacity of the body of the *S*. In order to see whether such standardization actually did take place we read the actual current

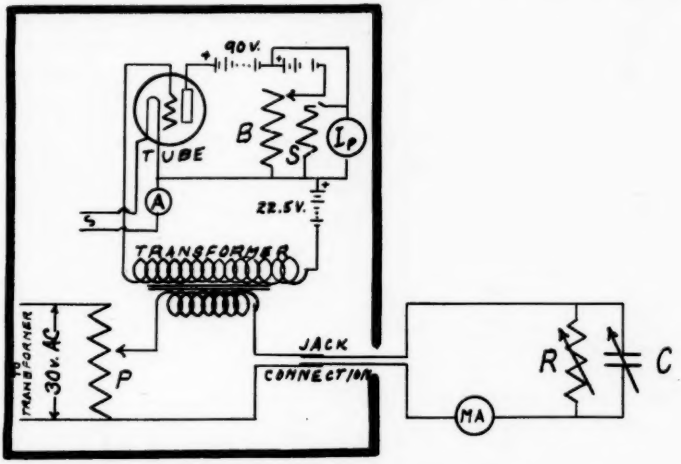

FIG. 1. SHOWING SIMPLIFIED DIAGRAM OF HATHAWAY CIRCUIT WITH R AND C IN PLACE OF S

Heavy line = Hathaway cabinet; A = filament ammeter; B = meter zero adjustment rheostat; C = variable capacitance; Ip = Hathaway milliammeter; MA = milliammeter; P = potentiometer; R = variable resistance; S = shunt.

passing in this external circuit on a rectifying milliammeter with a scale range of 0–1 ma. The physical resistance used was a General Radio Co. 111,110 ohm decade resistance box. The resistance windings in this box are of Ayrton-Perry and tape type, designed to be practically non-inductive and to have a very small distributed

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[11] K. S. Cole, Electric impedance of suspensions of spheres, *J. Gen. Physiol.*, 12, 1928, 29-36; Electric phase angle of cell membranes, *ibid.*, 15, 1932, 641-649.

[12] J. F. McClendon, The permeability and the thickness of the plasma membrane as determined by electric currents of high and low frequency, *Protoplasma*, 3, 1927, 71-81.

[13] A. Strohl, La conductibilité des organismes vivants en courant alternatif: II. Schémas équivalents aux conducteurs organiques, *J. Radiol. et d'Électrol.*, 15, 1931, 537-552.

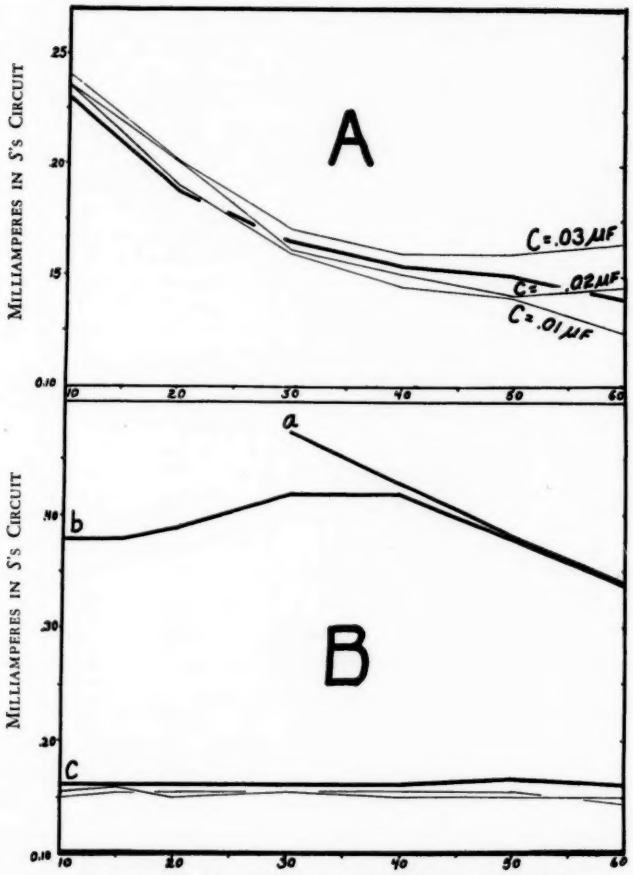

FIG. 2. SHOWING CURRENT THROUGH S FOR VARIOUS LEVELS OF BODY RESISTANCE AND CAPACITY

A = Apparatus No. 1; B = Apparatus No. 2; Heavy lines = current at approximated levels of body impedance; light lines = at various resistance levels and a given capacitance level. a = 1.0 amperes filament current; Ig = 0; no standardization possible. b = 2.05 amps. filament current; standard current in grid circuit = .05 ma.; indicating meter (Ip) could not be set to zero. c = 2.05 amps. filament, Ip (Shunted) = .05 ma. as standard.

capacity. The capacitance used was a General Radio Co. 0.001–0.1 microfarad decade capacitance.

In order that our results might be of general application, we have determined the characteristics both of an instrument assembled by Hathaway and of a Marietta Apparatus Co. outfit. These will be designated as Apparatus No. 1 and Apparatus No. 2. As far as we know, no other manufacturers have marketed this apparatus.[14]

The current flowing in *S*'s circuit is plotted against the level of resistance in Fig. 2. The heavy line in each half of this figure represents the value of current through *S*'s circuit when resistance alone was substituted for *S*, and therefore represents roughly the effect of impedance change.[15] The light lines show how this current varied with both resistance and capacitance in *S*'s circuit. The upper curve in Fig. 2A, for instance, shows that with 0.03 microfarad in circuit and a 10,000 ohm resistance in parallel with it, the current through the external circuit (through *S*) was 0.24 milliamperes, when the apparatus was set as directed in the accompanying instructions. The same curve in Fig. 2A shows that, with the same capacitance and 20,000 ohms in parallel with it, 0.20 milliamperes flowed through the circuit at the standard setting. It will be noted that the general shape of the curves is similar for the pure resistance and for the parallel combination of resistance and capacitance. Crossing of the curves probably indicates slight variations of A.C. main line current not taken care of by the ballast tubes of the apparatus. It is quite apparent from these curves that as resistance or impedance in *S*'s circuit varied from 60,000 to 10,000 ohms the current through *S*'s circuit varied roughly from 0.13 to 0.24 milliamperes for Apparatus No. 1. *In other words, the setting of the meter of this variety of apparatus at 0.06 milliamperes does not satisfactorily standardize the current in the external circuit.*

Fig. 2B shows similar curves for Apparatus No. 2. When the standardizing milliammeter on Apparatus No. 2 was thrown into the grid circuit we found it necessary to adjust the filament current to approximately 2.0 amperes instead of 1.0 ampere as stated in the directions, if we were to have the milliammeter record at all (curves a and b of Fig. 2B). Incidentally the same value of filament current (approximately 2 amperes) was necessary for sensitivity.

Curve b in Fig. 2B indicates that use of the milliammeter in the grid circuit for standardization purposes was generally unsatisfactory. Curve c shows that by using the shunted standardizing milliammeter in the plate circuit set to 0.05 milliamperes as a standardization index or by using the unshunted meter set to zero with the 'bucking out' control (B in the apparatus directions), a satisfactory constancy of current in *S*'s circuit was obtained. The latter method is recommended by Hathaway for maintaining the standardization after an initial adjustment has been obtained. Apparatus No. 2 was therefore superior in this respect to Apparatus No. 1.

Proportionality of deflection. In some studies it is desirable to compare the amplitude of the galvanic skin reaction of various *S*s to the same stimuli or of a given *S* to various stimuli. Now, if the apparatus produces any distortion in the

[14] The C. H. Stoelting Co. sold several instruments assembled by Hathaway but does not now make them.

[15] Since resistance equals impedance when there is no capacitative or inductive reactance.

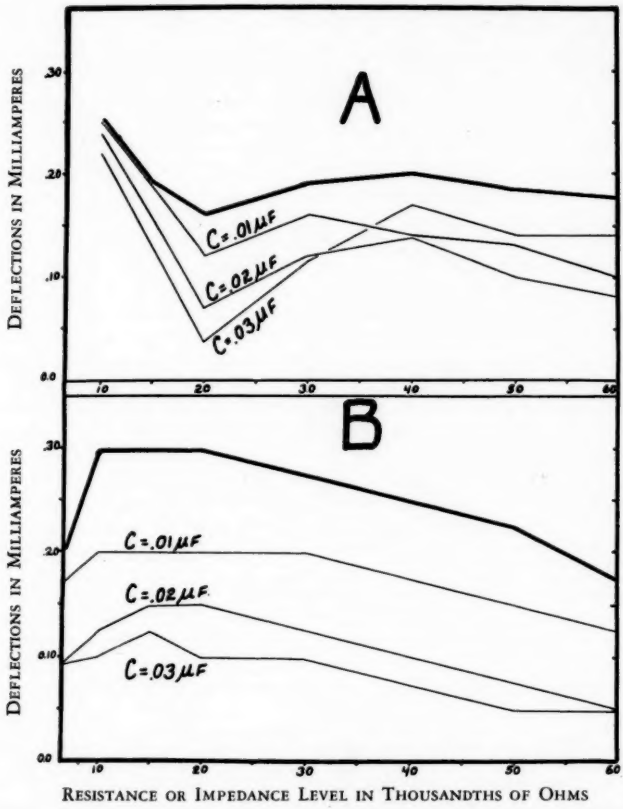

FIG. 3. SHOWING MAGNITUDE OF DEFLECTION PRODUCED BY 2000 OHMS CHANGE AT VARIOUS LEVELS OF BODY RESISTANCE AND CAPACITANCE

A = Apparatus No. 1; B = Apparatus No. 2. Heavy lines = for impedance approximation; Light lines = for resistance at given capacitance values.

amount of deflection, such distortion should be known and corrected. We were, therefore, interested in the relationship between recorded deflection and actual change in the impedance of *S*. (Impedance is in alternating current measurements, the counterpart of resistance in direct current measurements.)

In order to determine the relationship of amount of deflection to amount of impedance change, we again used the circuit shown in Fig. 1, standardizing both Apparatus No. 1 and No. 2 in the optimum manner for each type. Since McClendon

and Hemingway[16] have shown that both resistance and capacity of the skin vary on stimulation and since each of these produces a deflection on the Hathaway apparatus, it was desirable to vary each and also to vary their resultant, *i.e.* impedance.

Fig. 3 shows the deflections in milliamperes which were produced by a reduction of 2000 ohms resistance for Apparatus No. 1 and No. 2. The heavy lines represent determinations made by varying resistance alone, and therefore represent an approximation of the effect of impedance variations (see note 2 above). The heavy line in Fig. 3A indicates that with 10,000 ohms in the circuit, a reduction of this resistance to 8,000 ohms gave a deflection of 0.25 milliamperes on the indicating meter (Ip) of Apparatus No. 1. Each of the light lines represents the amount of deflection for the same resistance change (minus 2000 ohms) at each level of resistance when the capacitance was kept at a given value; *i.e.* 0.01, 0.02, 0.03 microfarads, respectively. Again it will be noted that the introduction of capacitance does not change the general form of the curve but does change its general level somewhat.

From these curves it will be seen that a given value of impedance or resistance change (minus 2000 ohms) of the parallel combination used in place of *S* gave very different amplitudes of deflection at different values of total resistance level. This means that with Apparatus No. 1 and an *S* whose body resistance is 20,000 ohms an actual reduction of 2,000 ohms in his resistance would result in smaller deflection than the same resistance change in an *S* whose level of body resistance is 10,000 ohms or one whose resistance is 30,000 ohms.

Fig. 3B shows similar curves for the amount of deflection produced by a reduction of 2000 ohms in Apparatus No. 2. A comparison of A and B of Fig. 3 shows that the magnitude of deflection from the two pieces of apparatus varies in a dissimilar manner. For both Apparatus No. 1 and No. 2 the deflection produced by 2000 ohms change varied in amplitude from 0.10 to 0.30 milliamperes as total resistance varied from 60,000 to 10,000 ohms.

It occurred to us that the deflection might be proportional to the percentage resistance change in *S* rather than proportional to actual resistance change. Fig. 4 demonstrates that this is emphatically not the case. In this figure the impedance curves of Fig. 3 have been replotted. Impedance change has been expressed as percentage of total impedance; and deflection has been put in terms of percentage of maximum deflection (that obtained at 10,000 ohms) for each piece of apparatus.

If the deflection were proportional to percentage of impedance change a given percentage change would always add the same amount of deflection. This would appear as a straight line sloping toward the upper left of the figure. Such a line appears only for Apparatus No. 1 between 10,000 and 20,000 ohms level.

The apparatus, therefore, produces a deflection which varies in proportionality to the absolute change or to the percentage change of impedance involved in the galvanic reflex. Now, if this variation of amplitude is to be corrected for, we must know the actual body impedance at the time of reaction. The Hathaway apparatus gives no such measure. We could make the correction on the basis of a *preliminary independent* determination of skin impedance *if* the level of *S*'s skin impedance remained the same during the course of the experiment.

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[16] J. F. McClendon and Alan Hemingway, Variations in the polarization capacity and resistance of the skin, *J. Gen. Physiol.*, 13, 1930, 621-626.

Skin impedance, however, falls during the course of an hour as is shown by Fig. 5. The curves show the course of skin impedance or resistance of 3 *Ss* during the course of an hour. Determinations of impedance were made by alternately placing *S* and the decade resistance across the electrode connections of the Hathaway apparatus. The substituted resistance which gave the same standard current on the Hathaway milliammeter (Ip) as that with *S* in series, was used as his body *impedance. Re-*

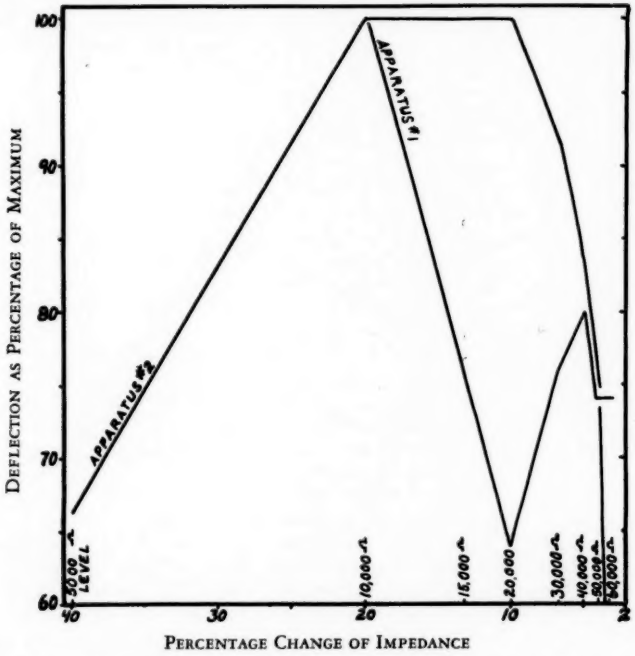

FIG. 4. SHOWING RELATION OF DEFLECTION TO PERCENTAGE CHANGE OF IMPEDANCE

Deflections shown by the heavy lines of Fig. 3A and B are re-plotted in terms of percentages.

sistance determinations were made on an A.C. bridge balanced by means of resistance and capacity in parallel using a 0 to 1 rectifying milliammeter as a null indicator. All curves for these *Ss* show a drop of skin impedance or resistance during the course of an hour; the drops being from 10,000 to 50,000 ohms in extent. Obviously, then, we cannot apply a correction to the Hathaway readings unless we make a *series* of impedance determinations during the course of the experimental period.

In order to make such a series of determinations, a microammeter might be used in series with *S* and Apparatus No. 1 calibrated by use of a physical impedance

somewhat in the manner we have used. Current through *S* would give a rough index of body impedance and therefore of apparatus sensitivity. Since current through *S* remains constant for Apparatus No. 2 when standardized, this method could not be employed here. The manufacturers could perhaps revise the circuit to give a measure of resistance in some such fashion as Davis has done with a D.C. vacuum-tube circuit.[17] If such determinations are not made and Hathaway scores are used uncor-

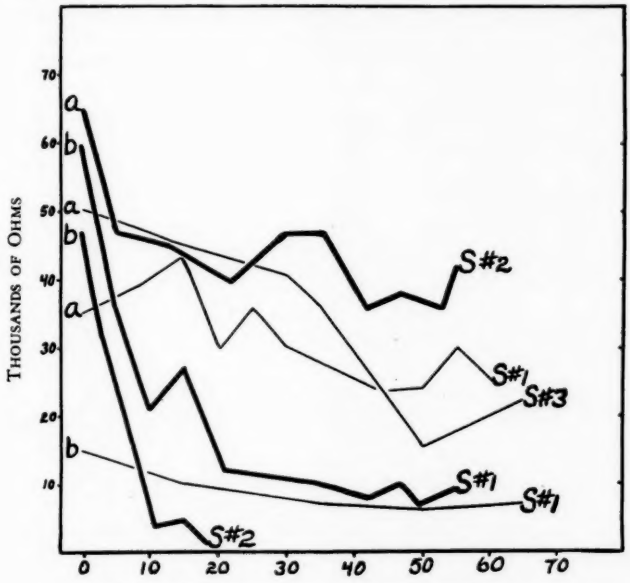

Fig. 5. Showing Variation of Skin Impedance and Resistance of 3 *Ss* During One Hour

Heavy lines = approximate impedance value; Light lines = resistance by bridge with 0.12 to 0.14 ma. through S. a = using small; b = using large Hathaway electrodes.

rected, ordinary statistical treatment of amplitudes will be invalid. Fig. 6 indicates how seriously the fall of body resistance may affect the amplitude of deflections. The resistance curve (a) for *S* 3 of Fig. 5 has been used, to construct Fig. 6, since it is intermediate in level and steepness. The amount of distortion is the vertical distance of each curve from its respective horizontal (dotted) at any given time.

In Fig. 6 actual deflection is plotted against actual resistance change (from apparatus curves of Fig. 3). Plotting percentage change only exaggerates the distor-

[17] *Op. cit.*

tion. The curves show rather dramatically that the same galvanic reaction would produce very different deflections for this *S* during the course of an hour. It is clear that an increased deflection may mean a greater galvanic skin response (G.S.R.), but it may indicate merely a lower level of body impedance or even (for Apparatus No. 1 especially) an increased level (cf. Fig. 3).

The labor of calibration and computation involved in applying a correction may make it more convenient to eliminate the Hathaway apparatus and use deflections and impedance determinations from a properly designed A.C. or D.C. bridge. Darrow,[18] for instance, has designed a very convenient, sensitive and well standardized D.C. bridge for psychological research measurements. Many investigators in the physiological field have used the A.C. bridge (cf. Landis).[19]

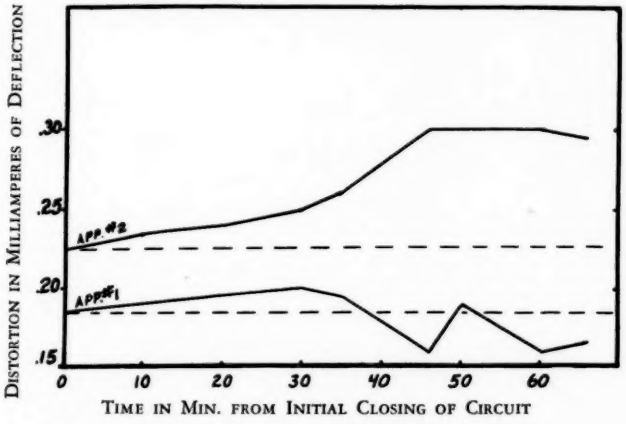

Fig. 6. Showing Distortion of Galvanic Response Due to Continuous Drop of Body Impedance

Computed for *S*3, Fig. 5, on the basis of Fig. 3A and B. Distortion is represented by vertical distance of each full line from its corresponding dotted line.

In general, therefore, the Hathaway galvanic reflex apparatus is of value for demonstrating the G.S.R. with alternating current but, as at present produced, should not be used for research requiring comparison of deflections except in some all-or-none fashion.

Summary

Recent studies using the Hathaway apparatus and the apparent growing popularity of the circuit, together with detailed statistical treatment of deflections therefrom, as in Crosland's study, have led us to determine the actual characteristics of the circuit.

[18] C. W. Darrow, Uniform current for continuous standard unit resistance records, *J. Gen. Psychol.*, 6, 1932, 471-478.

[19] C. Landis, Electrical phenomena of the skin (galvanic skin reflex), *Psychol. Bull.*, 29, 1932, 693-752.

(1) The characteristics were determined by putting in place of the subject a parallel combination of non-inductive and non-capacitative resistance and a decade capacitance (Fig. 1). Current through the subject's circuit was measured by means of a rectifying milliammeter, and deflections were noted for varying resistances and capacitances (R and C, Fig. 1).

(2) One Hathaway instrument made by each of two manufacturers was checked. To the best of our knowledge no other varieties have been made available.

(3) After setting the apparatus according to directions for standardizing of current through *S*, the actual current through *S*'s circuit was read. For Apparatus No. 1 the current through *S*'s circuit varied from 0.13 to 0.24 milliamperes as total resistance varied from 60,000 to 10,000 ohms. Constancy of current was therefore not obtained. For Apparatus No. 2, following the manufacturer's directions did not give satisfactory operation or standardization of current. A constant current in *S*'s circuit was obtained, however, by using a 2.0 ampere reading on the filament meter instead of 1.0 as directed, and by using the milliammeter in the plate circuit only.

(4) Both makes of apparatus showed a variation of deflection of from 0.10 to 0.20 milliamperes when a given impedance change (minus 2000 ohms) was made at different levels of total impedance from 60,000 to 10,000 ohms. The size of deflection was proportional neither to actual resistance change nor to the percentage of resistance change in *S*'s circuit.

(5) Three *S*s showed a drop of total resistance of from 15,000 to 50,000 ohms when attached to the apparatus or to an A.C. Wheatstone bridge during the course of one hour.

(6) It is therefore impossible to tell whether an observed increase of deflection represents an increased galvanic skin reflex (G.S.R.), or a change in the level of body resistance, unless a measure of total skin resistance level is also made at the time of the G.S.R. No facilities for such measures are at present supplied with the instrument. The manufacturers might be able to revise it to include such a measure somewhat as Davis has done with a D.C. circuit.

(7) The Hathaway apparatus is suitable for demonstration purposes, but, as now put out, should not be used where magnitude of deflection is desired as an index of magnitude of G.S.R.[20] Circuits are available for research use as designed by Darrow and Davis for D.C. and by various physiological investigators for A.C.

[20] The manuscript of this article was read by Dr. S. W. Hathaway. He agrees to the advisability of pointing out the character of the apparatus and calls attention to the fact that at the end of his study, he advocated the use of *frequency* of response without regard to quantitative value (*op. cit.*, 645).

IMPROVEMENT AND STANDARDIZATION OF THE PURSUITMETER TECHNIQUE*

By ALVHH R. LAUER, Iowa State College

The pursuit techniques of Miles,[1] Koerth,[2] and Weiss and Renshaw,[3] have had a rapid spread in interest. Such devices, originally designed for measuring sensori-motor learning, have been found applicable to studies of genetic behavior as well as to problems in the clinical and abnormal fields. With further advances in construction of synchronous motors the possibilities of improved types of instruments have become much greater.

While studying the relative influence of various factors of integrated response mechanisms the writer found need for an extension of the pursuitmeter equipment. Certain other tests were modified, assembled, and attached to the pursuitmeter as described in a previous study.[4] The wiring diagram is shown in the same paper.

The original instrument used by Weiss and Renshaw employed a spring handle stylus.[5] This had some disadvantages and a rigid stylus was tried. It proved equally unsatisfactory since *S* could ride the electrode in such a way as to slow down the motor and coast along with it. The kinaesthetic sensitivity of some *S*s seemed to increase their learning in a spurious manner. Others would barely touch the electrode and thus change the nature of the problem.

To offset these tendencies the writer devised the electrode and stylus shown in Fig. 1. The core of the electrode (K) is insulated from the cup (J). The latter two are wired in parallel with separate counters on recording circuits. When the stylus point (B) breaks contact it may either move off to one side or it may slide down over the oiled electrode and register an error by touching the cup (J). The elbow joint (*3*) prevents riding. The stylus is rounded to constitute the section of a sphere. It is kept lubricated by a fine oil. The shield (F) standardizes the distance of the hand from the electrode. A tripod support (D) carries the added weight of the cup and other accessory apparatus.

This is only one aspect of the changes introduced. A floating type of electrode (*5*) is attached by the nut (A) to the arm (*1*). The brass bracket (*5*) is located so that the hole (I) is directly over the electrode (K). The small part of the stylus (C) is held in the hole and must be kept off the side or an error is registered. If the stylus rides the side the motor stops. This constitutes a second type of serial

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* Developed during the tenure of a National Research Council Fellowship held by the author at the Ohio State University 1929-30 under the direction of Dr. A. P. Weiss. Credit is due Mr. Leon Goldstein for assistance in carrying out the study.

[1] W. R. Miles, A pursuit pendulum, *Psychol. Rev.*, 27, 1920, 361-376; The pursuit meter: An apparatus for measuring the adequacy of neuromuscular co-ordination, *J. Exper. Psychol.*, 4, 1921, 77.

[2] W. Koerth, A pursuit apparatus: Eye-hand coördination, *Psychol. Monog.*, 31, 1922, 288-292.

[3] A. P. Weiss, and S. Renshaw, Apparatus for measuring changes in bodily posture, this JOURNAL, 37, 1926, 261-267.

[4] A. P. Weiss, and A. R. Lauer, The psychological principles in automotive driving, *Ohio State Univ. Contrib. Psychol.*, 1930, (no. 11), 1-57.

[5] *Op. cit.*, 262 f.

response primarily involving eye-hand coördination. The contact-stylus technique involves more kinaesthetic and tactual cues. This is shown by the fact that induced visual defects do not noticeably affect the contact or ordinary pursuit performance, while they do affect the floating type.[6]

A third type of serial response mechanism is measured by adapting the Scott 3-hole test to the pursuitmeter. It moves in an irregular pattern in such a way as to vary the stereotyped performance of the stationary target. In experiments so far conducted a rate of 132 contacts a minute was established by use of a metronome and buzzer. The technique has great possibilities for investigating the 'personal tempo,' or optimal working speed of the individual in relation to learning.

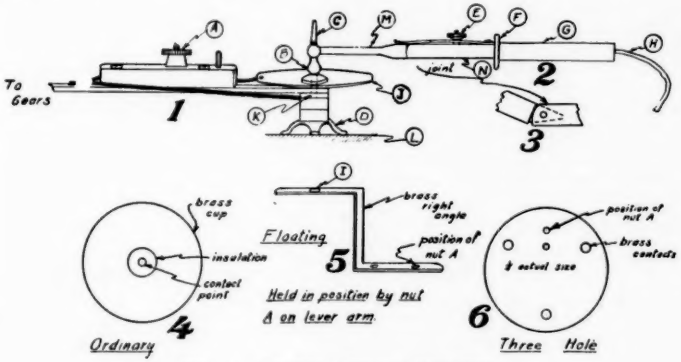


FIG. 1. SHOWING DETAILS IMPROVED

DESCRIPTION OF THE DIFFERENT MECHANISMS

(A) Ordinary. The diameter of the cup is $2\frac{15}{32}$ in. gradually curved to a saucer shape with the circumference $\frac{3}{16}$ in. higher than the center. The hole in center is $\frac{3}{8}$ in. with insulated plug $\frac{1}{2}$ in. in diam. at top, held tight by a $\frac{1}{8}$ in. pin (K) with lock nut under bracket support (D). This makes contact with the connection attached to (*1*), but is insulated from the lever.

The pin serves as the electrode. The 3 feet of bracket (D) describe an equilateral triangle $1\frac{3}{8}$ in. on a side. This runs on a fiber plate (L) of sufficient size to insure firm support at any extreme of the cycle-pattern. The distance from the plate surface to the extreme high point of the electrode is $1\frac{9}{32}$ in. The cup (J) is wired by a separate connection to another counter in parallel with that from the electrode. The ball (B) of the stylus is used as a means of contact, completing the circuit through the return connection (H). Thus both lost contacts and slips to the cup (J) are recorded.

(B) Floating. Bracket (*5*), which attaches at (A), has two holes $\frac{1}{32}$ in. in diam. At (1) a hole $\frac{1}{2}$ in. in diam. is used as a guide for the small end of the stylus (C). The latter is $\frac{7}{64}$ in. in diam. A contact of (C) and the bracket at (I) operates

[6] Weiss and Lauer, *op. cit.*, 96.

a relay which stops the motor in addition to registering an error. This eliminates riding. The bracket is made of silver plated brass 1¾ in. wide by 3/64 in. thick. The horizontal section at (I) is 1¾ in. long; the one at (A) 1 29/32 in. long; while the vertical section is 1 11/16 in., and is quickly detachable.

(C) Three-hole. The fiber disk (*6*) is ⅛ in. thick and 8 in. in diam. The two small holes attach it to the arm (*1*). The targets are silvered brass ½ in. in diam. and describing an equilateral triangle 5¼ in. on a side.

(D) Stylus. The stylus is 6⅜ in. over all from the tip of (B) to the attachment of (H). The dual stylus contacts (B) and (C) are attached by a screw tapped through the stylus handle. Contact (B) is 9/32 in. in diam. at the largest point and extends 11/32 in. Sections (M), (N), and (G) are 2 3/32, 1 19/32, and 2½ in. respectively. The small diam. at (M) is 3/16 in., while that at (N) is 5/16 in. (G) is covered by a soft rubber sleeve which fits up snugly to the fiber shield at (F). This standardizes the distance of the hand from the electrode. The stylus (*2*) is hinged at (*3*) by an elbow joint. The tension required at (B) to break the joint is approximately 70 grm. It can be regulated by the screw and spring at (E). Not more than 200 grm. pressure can be placed upon it.

A tapping device was also attached to the instrument, and by manipulating switches speed of movement records can be taken very easily. The timing device attached to the pursuitmeter is quite accurate within the limits of error of the type of synchronous motor used. This is accomplished by star-wheels. The technique was the same as that used by Ream,[7] except that 10 sec. tapping periods were given.

All contacts on the instrument are either silver or are heavily plated with silver. Silver oxide is a conductor, and therefore poor connections due to corrosion are practically eliminated.

The reliability of the tests as measured by correlating the mean of the first and fourth trials with the mean of the second and third trials was found to be as follows, after correction by the Spearman-Brown formula: ordinary pursuit, R = +0.939; three-hole pursuit, R = +0.895; floating pursuit, R = +0.906.

The correlation between the three types of pursuit movement is not high. An *r* of +0.435 was obtained between the ordinary and the floating type of performance. Between the 3-hole and the ordinary, *r* is —0.149, while between the 3-hole and floating, an *r* of —0.148 was obtained. The high score, in terms of contacts made on the ordinary and floating performance, is low score. The actual correlations are therefore slightly positive.

The three types of pursuit movement, together with the speed of movement tests, make an ideal battery for measuring sensori-motor and serial learning. In general the performance is as follows: first trial with the preferential hand, second trial with the non-preferential hand, third trial with the non-preferential hand, and final trial with the preferential hand. This order was adopted to offset fatigue and practice effects. By starting with the preferential hand *S* does better and is more strongly motivated. Each cycle is about 2 min. in length.

The pursuitmeter proper was built according to the specifications of Weiss and Renshaw.[8] A 1/100 h.p. constant speed motor was used instead of the ordinary type.

[7] M. J. Ream, The tapping test: A measure of motility, *Psychol. Monog.*, 31, 1922, (no. 140), 290-319

[8] Weiss and Renshaw, *op. cit.*

It is rated at 1800 r.p.m. instead of 1750. On 60-cycle alternating current the variation is slight unless overloaded. These techniques have been used for three years in our laboratories and they are relatively trouble-proof.

Validity of pursuit measurements has been covered by various writers cited in the literature. The reliability is very satisfactory for most experimental purposes. Because of slight differences in construction it is suggested that each instrument be standardized after it has been in operation for a short time by giving it to a representative group of *Ss*.

A THYRATRON-TUBE CHRONAXIMETER

By FREDERICK C. THORNE, Columbia University

The present apparatus has been developed in an attempt to produce a rectilinear electrical wave of a variable short duration for the purpose of testing the excitability of nerve and muscle preparations in chronaxy measurements. In most of the previous experiments, Lapicque's condenser discharge method has been used in preference to the Weiss ballistic rheotome or the Lucas pendulum because of its greater accuracy and ease of manipulation. A rectilinear impulse is obtained from the rheotome and the pendulum, as compared with the exponential curve derived from condenser charge or discharge. Although Lapicque has published results indicating that the three methods are essentially equivalent,[1] there remain several considerations in favor of the use of a square wave. Since the duration of the condenser charge or discharge is a function of the resistance across which it flows, it would seem that much precaution should be taken to insure a constant value for the resistance of the stimulated tissue from experiment to experiment.

A circuit giving a rectilinear wave has been made possible by the recent development in the General Electric laboratories of the Thyratron vacuum tube. The FG-17, described as a mercury-vapor, grid controlled rectifier, is recommended here because of the small voltage drop from filament to plate and also because once the grid has lost its control, it exerts no further appreciable influence on the plate circuit until control is returned by breaking the plate circuit.

In Fig. 1, at the starting point, there is a negative grid bias on Tubes I and II, this bias preventing any flow of current in the plate circuit of either tube. When switch 1 is closed, the grid bias is effectively removed from Tube I instantaneously, while the condenser C_1 delays its removal from Tube II for a short period of time dependent upon the value of C_1. The plate resistance of the tubes is very low, and it will be seen that there is a relatively high resistance in the plate circuit of Tube I, while there is none in the plate circuit of Tube II. When Tube II is in operation, it will short-out Tube I, since most of the plate current from the common B-battery will follow the path of least resistance through Tube II. Tube I therefore operates only in the interval between the closing of the switch 1, and the flick-on of Tube II which has been delayed by the condenser C_1. In any individual set-up, Tube II may not entirely short-out Tube I; this should be remedied by bucking out the remaining current with a small battery placed in the plate circuit of Tube I.

[1] L. Lapicque, *L'excitabilite en fonction du temps*, 1926.

The stimulating current is led off the potentiometer R_6, through which current flows only when Tube I is in operation. Switch 3 is opened only when it is desired to measure the voltage being led off from the potentiometer R_6, Tube II being inoperative in this case, therefore allowing Tube I to remain on long enough to permit a reading of the voltmeter V. A voltmeter of high internal resistance is specified in this circuit in order that its inclusion will not seriously alter opera-

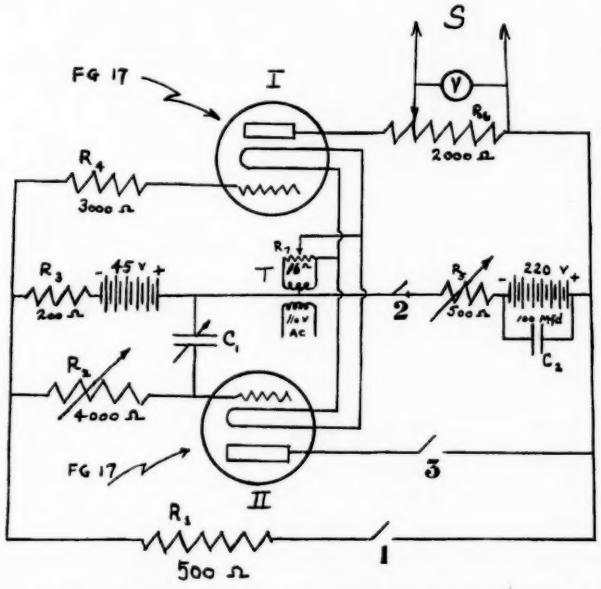


FIG. 1. CIRCUIT DIAGRAM FOR THYRATRON-TUBE CHRONAXIMETER

R_1, R_3, R_4 = fixed resistances of the non-inductive type (Lavite Resistors recommended); R_5, R_6, R_7 = variable tubular rheostats of ordinary type; R_2 = a calibrated resistance box of the decade variety; C_1 = a calibrated decade condenser (No. 219 G manufactured by the General Radio Co.; C_2 = a fixed electrolytic condenser (Dubilier Electric Co.); V = a Jewell D.C. voltmeter, pattern No. 2, 0 to 150 volt range (specified because of its high resistance; and T = the filament transformer (must deliver at least 10.8 amperes at 2.5 volts).

tions. Switch 2 is customarily kept closed except when it is desired to return the apparatus to the starting place by cutting off the plate circuit and restoring grid control. The value of the condenser C_2 is not exact; however, it should be as large as possible since this condenser is included to counteract the influence of any stray inductance that may be present in the circuit. In operation, it is advisable to leave all resistances except R_6 in a fixed position, since any change in these will cause the condenser to charge at a different rate. However, if the settings

of C_1 should not provide fine enough time intervals, the value of R_2 may be lessened, making possible an indefinitely small delay.

The filaments should preferably be operated from an A.C. transformer capable of delivering 10.8 amperes at 2.5 volts. This voltage should be accurately maintained using a filament voltmeter connected directly across the filament terminals of one of the tube sockets. The return leads from the plate and grid circuits should be connected as indicated to the midtap of the filament transformer.

The length of the stimulating impulse may be calibrated theoretically or at any single stimulation. It will be seen that by connecting a simple ballistic galvanometer in the plate circuit of Tube I, the quotient of the voltage on the load and the reading of the swing of the galvanometer will give the time of the impulse, assuming the wave to be rectangular.

AN APPARATUS FOR RECORDING FETAL MOVEMENT

By L. W. SONTAG and ROBERT F. WALLACE, Antioch College

In preparation for the investigation of the activity of the human fetus during the later half of its intrauterine life, we were confronted with the fact that no entirely satisfactory apparatus has been devised for recording the fetal movements. Since we wished to study the variation of movement from day to day, its relationship to fatigue, excitement, periods of the day, etc., it was necessary to record continuously for relatively long periods. There should be a minimum of inconvenience in the preparation for the taking of each record, and infrequent need for changing the record-sheets. Since there are movements of the maternal abdomen, particularly respiratory movements, which are in no way associated with fetal movement, it was essential that the records differentiate these from the fetal movements. Finally, it is necessary to record simultaneously motion of all parts of the maternal abdomen if the record is quantitatively to estimate the movements of the fetus.

Of the experimenters who have worked with fetal movement, two describe apparatus for recording it. Hicks used an apparatus which he described as "an instrument like a cardiograph" which he "tied lightly on the abdomen of a person lying on the back."[1] The cardiographic record on kymograph paper showed the respiration-wave upon which was superimposed the fetal movement. From the records which Hicks published, it is difficult for us to differentiate the fetal movements from others. He observed that movements of the mother's limbs will give the same record as fetal movement and must be watched. He also points out that tympany may cause similar movements of a coil of distended intestine. Ray used a 2-in. tambour, placed diaphragm-side-down on the mother's abdomen and fastened in place with adhesive tape.[2] In this method the tambours rise and fall with the

* From the Samuel S. Fels Research Institute, Antioch College, Yellow Springs, Ohio.

[1] J. B. Hicks, On recording the fetal movements by means of a gastrograph, *Trans. Obstet. Soc. London*, 22, 1880, 134.

[2] W. S. Ray, A preliminary report on a study of fetal conditioning, *Child Dev.*, 3, 1932, 175-177.

breathing movements of the abdomen and Ray states that the record showed only fetal movements. He used three tambours to record the movement in three parts of the abdomen, whereas Hicks used but one. In our opinion, however, the area of the three 2-in. tambours was not large enough to record a dependable part of the fetal movement. Recording in both of these studies was by kymograph.

The apparatus devised by the authors, with the aid of advice and suggestions of the late A. P. Weiss, is constructed as follows. It consists of four rubber sacs, each forming one quadrant of an ellipse, and together covering the entire abdomen. The four sacs are enclosed in a cloth bag to keep them in proper spatial relation to one another. Over the cloth bag containing the four sacs is placed a previously prepared thin plaster-of-Paris cast. A binder of unbleached muslin is passed around the patient's back and the ends laced together over the cast, thus holding the cast and air sacs firmly against the abdomen. (See Fig. 1.)

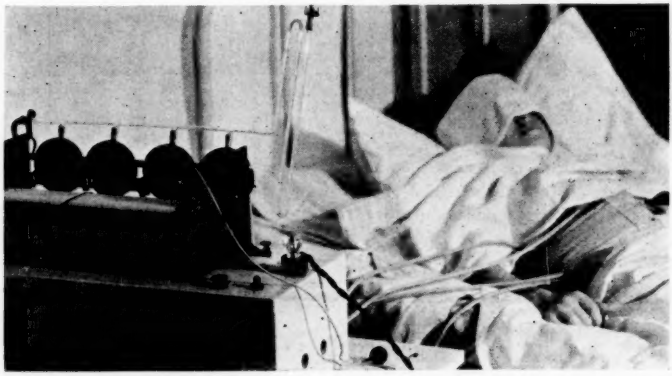


FIG. 1. APPARATUS FOR RECORDING FETAL MOVEMENT

The recording part of the apparatus (Fig. 2) consists of (1) four 3-in. tambours with diaphragms so mounted that there is no radial strain, (2) four pens each actuated by a short rod connected to the center plate of each diaphragm and passing out through the brass tambour-housing, (3) a 3-in. motor-driven brass drum which drives the paper through the machine, and (4) a brass table over which the paper is pushed and upon which the pens rest.

When the apparatus is in use, the four sacs, which have been connected with the four tambours, are inflated with air at a pressure of 60-120 mm. of water. Movements of any of the four quadrants of the patient's abdomen produce displacement of the respective tambour diaphragms and consequently excursions of the pens. As the brass drum revolves, it carries past the pens a strip of paper 14 in. in width fed from a roll suspended beneath the drum. The speed of the drum and the tension of the tambour diaphragms are both variable. The apparatus also includes valves and manometer necessary for obtaining the desired pressure in the system. Abdominal breathing of the mother is recorded by the even swinging of the four pens in unison. A fetal movement is indicated when the pens shift their

relative positions in such a way that one or more are going in one direction at the same time that others are going in the opposite direction.

The apparatus has been in use for a total of about 60 hours during the past three years. It is being used for the study of fetal conditioning as well as in an attempt to ascertain the relation between fetal movement and maternal fatigue, emotional states, hunger, and ductless gland-secretion. Its advantages are briefly as follows: (1) it uses ordinary paper and barographic pens rather than the smoked paper of the kymograph and so is capable of furnishing a continuous record for several hours; (2) it is propelled by an electric motor rather than a spring driven motor; (3) it has a recently developed tambour in which the tension of the membrane is obtained by a spring instead of radial tension in the membrane itself;

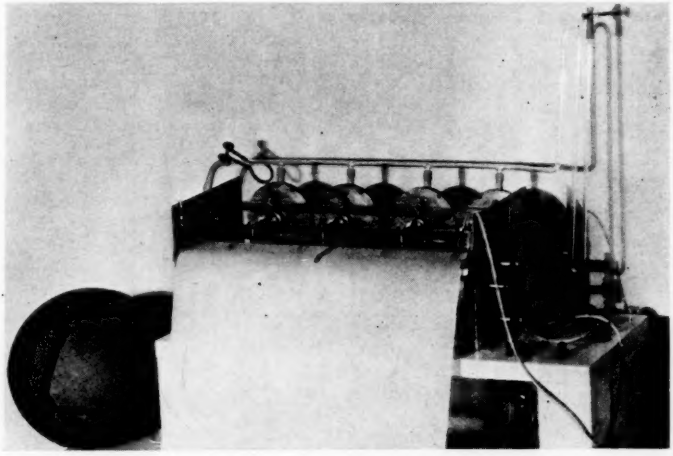


FIG. 2. RECORDING APPARATUS

(4) variation of the pressure against these diaphragms may be obtained by changing and regulating the compression springs; (5) the recorder has four tambours and is therefore capable of recording, independently and simultaneously, movement of four variables; and (6) with minor changes, it may be used for recording such other factors as heart beat, respiration, and tremors.

A RESET DIAL FOR CHRONOSCOPES

By L. F. BECK, Brown University

Several dial-type chronoscopes, such as the Hipp, the Cenco impulse counter, and under certain conditions the Dunlap, carry no convenient reset mechanism for the sweep-second hand. This limitation forces one to note the initial as well as the final readings of the hand during a series of measurements. Such an operation

is both cumbersome and time-consuming. This objectionable feature of the dial-type chronoscope can be overcome by supporting an adjustable, secondary, graduated dial on the face of the instrument. Prior to a measurement the dial is turned so that its graduated zero line is directly over the tip of the hand of the chronoscope. After the reaction the number of units is read directly from the dial; whereupon the dial again is turned until its zero line is coincident with the hand of the chronoscope.

The following detailed description is confined to such a reset dial adapted to the Cenco impulse counter. This instrument is selected as an illustrative model, since it can be used either as a timer or a counter, and therefore is applicable to many

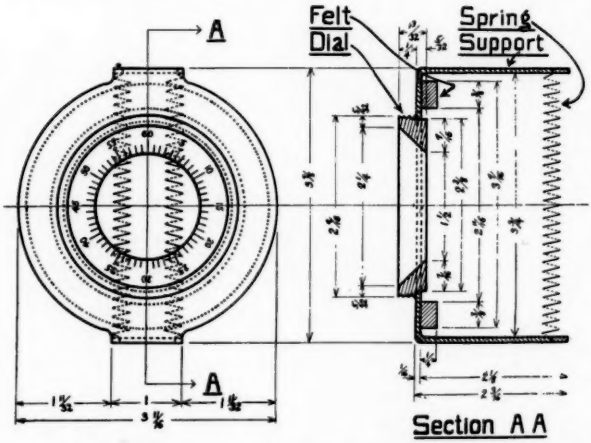


FIG. 1. ASSEMBLY SKETCH OF RESET DIAL FOR THE CENCO COUNTER

kinds of reaction-experiments.[1] Specifications for the dial and its support are given in the assembly sketch, Fig. 1. The support is fashioned from 1/16 in. sheet brass. The diameter of its aperture is 2 25/64 in., 1/64 in. larger than the bottom rim of the dial. The support is attached by spreading the two 1/4 in. springs until they encircle the cylindrical metal cover of the counter. The support then is pushed down until the felt (or rubber) pad rests upon the top surface of the counter.[2] The dial is cut from 2 3/4 in. brass rod. Its outer top rim carries a no. 5 knurl. The bottom rim is 5/32 in. deep, which is 3/32 in. more than the thickness of the support calls for. It is considered expedient to preserve this dimension in order that, if one wishes,

[1] For a discussion of some of the numerous uses and a description of this counter, cf. W. R. Miles, Correlation of reaction and coördination speed with age in adults, this JOURNAL, 43, 1931, 377.

[2] It has been found that the dial support can be centered more accurately above the face of the counter if it is provided with four legs instead of two as depicted in the assembly sketch in Fig. 1.

the dial can be mounted on a panel, and the counter concealed below it in a cabinet. The graduations of the dial correspond to those on the face of the counter. The cutting of the graduations on the inside bevel can be done with a Brown and Sharpe milling machine equipped with a spiral head. Both the dial and the support are chromium-plated, and in this respect they match the metal cover of the counter.

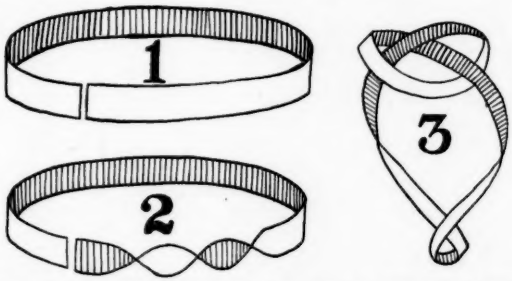
The dial does not obscure the graduations on the face of the counter. In fact, readings can be taken from the graduations on the dial or on the counter with equal facility. The dial, however, embodies the advantageous reset principle.

APPARATUS NOTES

An Experiment in Reasoning

The following problem, which has an unknown history, may be used as material for an experiment or exercise in reasoning.

Cut a small strip of paper, about 2 cm. wide and 25 cm. long; bend the strip so that the two short ends come into contact (Fig. 1); turn one end three times 180° about the longitudinal axis of the strip (Fig. 2); paste the two ends together; cut the strip to half its width by cutting along the middle of its entire length. A strip will result containing a well-formed knot (Fig. 3)—one that customarily results from inserting a free end through a prepared loop. The problem is to anticipate the result and to explain it after it has been obtained.



Figs. 1-3. Showing the Strip, the Twists, and the Knot

Similar phenomena were demonstrated at spiritistic meetings in Germany 50 years ago and were given as evidence of the supernatural. Shortly after, Simony wrote a book upon the formation of knots in an enclosed circular band;[1] and more recently Höfler and Witasek included the problem as an exercise in their manual of psychological experiments.[2] Nothing further is known about the history of the problem. Its discoverer is obscured by its spiritistic background.

University of Southern California Norma V. Scheidemann

[1] Oskar Simony, *In ein ringförmig geschlossenes Band einen Knoten zu machen*, 1881.

[2] Alois Höfler and Stephen Witasek, *Hundert psychologische Schulversuche*, 1903 (Exper. No. 38), 17.

NOTES AND DISCUSSIONS

A Critique of Crosland's "Measurement of Emotion"

The development of the "ready made psychogalvanic apparatus," such as that of Hathaway,[1] makes possible the use of the technique by any experimenter, regardless of his technical knowledge of the apparatus or of this field of research. This is an undoubted advantage for general classroom work, but in research it has led to fictitious results and fallacious interpretations.

Certain errors in technique and interpretation in the most elaborate study which has utilized the Hathaway galvanic apparatus will serve as an example of this scientific error. The following critical remarks apply equally to other studies in which similar errors appear. In a preceding paper, Forbes has presented the electrical characteristics of the Hathaway apparatus.[2]

Crosland's study[3] presents a detailed statistical treatment of results which are in great part invalidated by certain fundamental errors in his use of the apparatus. For instance, a great deal of space and statistical analysis is devoted to evaluation of "absolute" versus "relative" readings. These two types of readings are described in his monograph as "observing merely the whole deflection and recording the farthest point reached by the needle," and as "observing and noting down the differences between the beginning point and the termination-point of the deflection."[4] An understanding of the apparatus would have eliminated consideration of "absolute" readings or else would have resulted in a different interpretation of the statistics obtained with them.

Crosland was greatly surprised to find the absolute readings more reliable statistically, but later discarded them on the basis of low validity. He suggested that the difference between the two measures represented "persisting reaction."

Interpretation on the basis of a "persisting reaction" seems a bit ambiguous. If this means an incomplete recovery of the skin in returning to a normal impedance level due to a too short time interval between stimuli, the technique is open to question as will be shown below. On the other hand, if a gradual change of basic resistance level is meant, then the phenomenon is similar to the well known "rest curve" of direct current galvanic skin measures which is ordinarily corrected for by resetting the resistance units of the bridge or other measuring circuit. The Hathaway apparatus is provided with a potentiometer adjustment for this purpose and reference to Hathaway's work shows that his "absolute readings" started from approximately zero in each case.

(1) In Crosland's work this adjustment was evidently not used properly, *i.e.* the milliammeter needle quite evidently did not start from zero, for Crosland says,

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[1] S. R. Hathaway, A comparative study of psychogalvanic and association time measures, *J. Appl. Psychol.*, 13, 1929, 632-646.

[2] T. W. Forbes, The Hathaway galvanic apparatus as an instrument of research, this JOURNAL, 45, 1933, 502-511.

[3] H. R. Crosland, Objective measurements of emotion, *Univ. Oregon Pub.*, 1, 1931, (no. 3), 153-178, 179-196.

[4] Crosland, *op. cit.*, 157.

"the beginning points of the deflections were the most variable."[5] Table I shows 8 scores from Table XIII of the monograph which have been picked to illustrate the fact that the adjustment was not properly made. Columns 1 and 2 give respectively the "absolute" and "relative" readings from Crosland's table. Column 3 is the average starting point of the needle which we obtained by subtracting the average "relative" values (column 2) from the average "absolute" (column 1). Column 3, therefore, represents roughly the adjustment of the meter. The adjustment levels computed, vary from approximately 0.10 to 0.80. Since the scale on the Hathaway meter runs from 0.00 to 1.00 ma., it is evident that these average adjustment levels represents a wide difference from *S* to *S*. Either the meter adjusting potentiometer was set differently for different *S*s, or else the initial drop of resistance level was allowed to go uncompensated. Forbes demonstrated a continuous drop of resistance in impedance level of 3 *S*s who were sitting quietly with as little in the way of

TABLE I

SAMPLE READINGS TO SHOW VARYING STARTING POINTS (METER ADJUSTMENTS) FROM CROSLAND'S TABLE XIII

	Subject No.	Non-significant			Significant			Post-significant		
		1 Absolute	2 Relative	3 Start	1 Absolute	2 Relative	3 Start	1 Absolute	2 Relative	3 Start
visual series	39	.106	.038	.068	.083	.050	.033	.079	.044	.035
	42	.860	.068	.792	.868	.099	.769	.860	.066	.794
	46	.574	.072	.502	.722	.185	.537	.645	.047	.598
	50	.130	.028	.102	.120	.052	.068	.122	.031	.091
oral series	10	.778	.075	.703	.759	.091	.668	.762	.085	.677
	12	.197	.070	.127	.261	.093	.168	.260	.074	.186
	14	.866	.075	.791	.893	.110	.783	.876	.093	.783
	38	.672	.072	.600	.714	.098	.616	.702	.097	.605

stimulation as could be easily obtained.[6] There were wide individual differences even under these conditions. There is no evidence, therefore, that a similar drop in level can validly be referred to as a "persisting reaction" to association stimuli.

In any case, the effect would be to add a constant error to each individual's score. This constant, as will be seen from our Table I, is in most cases a large percentage of the *S*'s "absolute" score and for each individual is fairly constant (see Table I, Non-Significant, Significant, and Post-Significant), hence a high reliability coefficient based on an error of manipulation. The use of elaborate statistics on faulty data is, of course, rather pointless.

(2) A second criticism lies in the rate of presentation which introduced an uncontrolled variable. The automatic phonograph presentation allowed a maximum of 7.7 sec. interval between stimuli. Figures given by Davis,[7] and records in our laboratory, indicate that the total cycle of a galvanic reflex often requires longer than 8 sec. Evidence that such cases occurred in his study appears in Crosland's

[5] *Ibid.*, 159.
[6] Forbes, *op. cit.*, 508ff.
[7] R. C. Davis, Factors affecting the galvanic reflex, *Arch. Psychol.*, 18, 1930, (no. 115), 1-64.

monograph, since "the P.G.R. reaction was usually in progress just at the instant when the next succeeding stimulus was being presented."[8] Therefore the effect on readings of the overlap is unknown and uncontrolled, and comparisons of magnitude of deflections are accordingly invalidated.

(3) In the third place, several factors tend to invalidate the statistical findings. Crosland's significant stimulus series were divided in such a manner that 15 of the 20 significant words fell in the last half of the list, thereby displacing words of the non-significant list. Therefore there were three times as many significant reactions in the last half of each stimulus series as in the first half. There were, on the other hand, slightly *less* than 50% of the total number of non- and post-significant reactions. Any factor which produced a progressive increase in the magnitude of deflection during the presentation of a list would, therefore, have increased the average of the significant reactions to a greater degree than the average of the non- and post-significant ones.

Such a factor would be expected to have operated. Davis has called attention to the fact that the deflection is known to increase with the current used, and Forbes has shown that a gradual drop of *S*'s body impedance may occur when the *S* is attached to the Hathaway apparatus. Forbes also showed that, in one form of the Hathaway apparatus, the current through *S* increased as body resistance decreased. Crosland obtained deflections to relevant stimuli which were significantly larger *statistically* than those from irrelevant and post-relevant. Since no check was made of the extent to which increased deflection was due to decreased body resistance and resulting increase of current through the body, we do not know how much of this difference represents characteristic reaction to the two types of stimuli. Until we do, the fact that the difference was statistically significant cannot be taken at face value in drawing conclusions.

(4) Furthermore, the *Hathaway apparatus itself* produces a distortion which makes comparisons of deflection magnitudes generally undesirable, for, as Forbes has shown, the proportionality of deflection to galvanic reaction not only differs, but it differs in a non-linear fashion. If, then, the body resistance of a given *S* changes during the sitting, our determinations indicate that the apparatus itself introduces a gradually changing (increasing for the most part) magnitude of deflection. This distortion by the apparatus makes the meaning of the greater deflections to relevant stimuli obtained by Crosland for any given subject still more ambiguous.

The same characteristics of the apparatus will also affect *individual differences* of deflection. If, for instance, one *S*'s body resistance is generally higher due to starting at an initial value of about 50,000 ohms while another's starts with an initial value of 30,000 ohms (see body impedance curves), then the distortion by the apparatus will affect quite differently the average scores for the two *S*s. It will, in general, produce a greater magnitude of deflection for the second *S* than for the first.

Hathaway apparently attempted to avoid comparisons between individuals on this account in correlating average scores for each word with reaction-times. Crosland criticizes Hathaway's technique on statistical grounds but in correlating the

[8] Crosland, *op. cit.*, 159.

average deflections and average reaction-times of each person on the significant stimuli, Crosland neglects to take into account the factor we have just mentioned, *i.e.* the individual differences in amplitude due to different body impedance levels. He also neglects the fact that such differences would increase the reliability coefficient for his "relative" scores in a spurious fashion.

Crosland mentions that larger differences of deflection occurred between different *Ss* than between different stimuli, indicating that such widely different body impedance levels probably did occur.[9] His finding of practically no correlation of P.G.R. with average reaction-time for relevant stimuli may, therefore, be valid or it may indicate that the *Ss* showing a given reaction-time score were equally distributed over the range of apparatus sensitivity as determined by body resistance.

The small negative relationship with the Thurstone Personality Scores is based on a similar correlation of average deflection for significant stimuli. The same criticism as above, again applies, and the coefficient may indicate a relationship to body resistance level rather than to galvanic reactivity.

In general then, the above factors, *i.e.* the unknown extent of overlap of reactions due to speed of presentation, the effect of different levels of body resistances, of *changing* body resistances, and of apparatus distortion, would tend to prevent the occurrence of a normal distribution of galvanic scores. Nevertheless, differences between means based on theoretically normal distributions are used in comparing visual, oral and phonographic presentations.

Moreover, the *actual* size of average differences ranged from 0.004 to 0.008 ma., the larger figure being but slightly greater than the probable accuracy of reading of the meter. In our experience this error is likely to be at least 0.005 ma. or about ¼ of one division on the meter. We therefore agree with Crosland that "they (the differences) are nowhere removed entirely from the possibility of being chance differences,"[10] and are surprised at their inclusion in the summary of results where it is stated that "small but evident differences accrued as between the three series."[11] We submit that the disturbing factors must be controlled, or else corrected for specifically by statistical means before such small differences can carry any weight. As Forbes pointed out in the previous paper, the Hathaway apparatus, *as at present supplied on the market,*[12] does not permit the application of such a correction.[13]

Summary

In recapitulation we have made the following criticisms of Crosland's study which should be understood as applying to any of the other galvanic reflex studies in which similar Hathaway procedure was used.

(1) Based on poor understanding or manipulation of Hathaway apparatus.

(a) Interpretation of the "absolute" deflection as containing a "persisting reaction" seems both ambiguous and questionable in the light of the progressive drop of impedance level long known in direct current studies and reported for alternating current measurements by Forbes.

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[9] *Ibid.*, 187. [10] *Ibid.*, 169. [11] *Ibid.*, 176.

[12] Crosland says, "The galvanometer employed was the market variety put out by Hathaway" (*op. cit.*, 154).

[13] Forbes, *op. cit.*, 505ff.

(b) As pointed out by Forbes, when using the Hathaway apparatus, comparison of magnitudes of deflection is invalid, since there is a varying proportionality of deflection with changing body impedance, unless special provision is made for calibration and correction. No such correction is reported by Crosland.

(2) Based on previous skin reflex work and general considerations.

(a) The high reliability of "absolute" deflection readings was apparently due largely to faulty manipulation of apparatus.

(b) The use of a list of significant stimulus words unbalanced as to position in the series would tend to accentuate differences due to increasing magnitude of deflection as general resistance level falls.

(c) The rate of automatic phonographic presentation was so fast as to introduce an overlapping of reactions to a varying and unknown extent, thus further invalidating comparison of magnitudes.

(d) Actual differences between group average deflections ("relative") of from 0.004 to 0.008 ma. were used for drawing conclusions. The probable accuracy of reading is about 0.005 ma. For this reason and in the light of criticisms above, we question the significance of such small actual differences no matter what their relationship to the magnitude of a probable error based on a theoretical normal distribution.

(e) In correlating total deflections with total reaction time scores on Significant, Non-Significant, and Post-Significant, for each individual, the effect of differences of magnitude occurring with individual differences in body resistance has been overlooked.

(f) The tendency to a spuriously high reliability for "relative" galvanic scores which would result from these individual differences, has not been considered by Crosland.

(g) On the basis of the above considerations, the alternative that "emotional and attentional reactions came through the galvanometer undifferentiated . . . or that . . . time records are influenced by attentional and intellectual perturbations but the psychogalvanic reactions are not so influenced,"[14] must be viewed as without experimental foundation.

(h) The results of the "psychogalvanic" sections of Crosland's study may be briefly and correctly stated as follows: Word association-reactions are accompanied by galvanic reactions when the Hathaway alternating current circuit is used.

Columbia University

T. W. Forbes
Carney Landis

Intelligence and Handedness

A number of attempts have been made to relate intelligence and handedness. Binet and Vaschide, for example, obtained a significant correlation between dullness and ambidexterity;[1] and their results were corroborated by Smedley, who observed that dull children tend to be equally strong in both hands.[2] Schuyten likewise found

[14] Crosland, *op. cit.*, 182.

[1] A. Binet and N. Vaschide, Experiences de force musculaire et fond chez les jeunes garçons, *Année Psychol.*, 4, 1897, 15-63.

[2] F. Smedley, *Report of the Commissioner of Education*, 1902, i, 1095-1115.

a definite relationship,[3] and Carman has written that intelligent children exceed dull children "by an average of 3 kg. with the right, and 1 kg. with the left hand."[4] Barr has observed that "a test of grip is more serviceable than a test of language to diagnose mental status."[5] MacDonald, however, found no evidence to support these conclusions.[6] Yet Wallin, who has studied many epileptics and feeble-minded subjects, believes that "degenerates tend to be ambidextrous, and that dextrality and intelligence go together."[7] The controversial nature of the supposition of correlation is indicated in these studies on the problem.

Since every entering student at Bucknell University is given an intelligence test and, among other physical tests, a strength of grip test, advantage was taken of the opportunity to correlate for these older subjects the ratings in these two tests. The records of men students in two successive freshman classes (339 in number) were taken for study. The average age at the time of testing was 18.56 years. No other study of the problem has dealt with a group at this age-level.

The intelligence rating was determined by the American Council Psychological Examination, 1931 and 1932 editions, by converting the raw scores into percentile values as given in the official reports on the test results for the two years concerned. Strength of grip was measured by the Smedley hand dynamometer, according to the technique described by Whipple.[8] Wallin, it should here be noted, suggests the possibility of inaccuracy in the Smedley instrument;[9] hence a check was made to ascertain the reliability of the strength of grip measurements. Fifty cases were taken at random and tested one month after the physical examination; the coefficient of correlation between the first and the second scores was $0.87 \pm .17$. Consequently it was concluded that the technique gave reasonably accurate results for the purposes of this study.

The data were treated from three aspects. (1) A correlation was drawn between strength of grip in the right hand and percentile ranking in the psychological test. The coefficient between these variables computed by the Pearson method was only $0.02 \pm .03$—not a significant relation.

(2) The dynamometric score for the left hand was then compared with the intelligence score. Here also the coefficient was low, being $-0.15 \pm .04$. There appeared to be no evidence to conclude that the strongest are the brightest, as Schuyten inferred from a study of children. This point may be illustrated by stating that the median dynamometric score in the right hand was, for those in the 90 percentile on the psychological test, 56.5 kg.; and for those in the 10 percentile, 58.1 kg. The superiority of 1.6 kg. of those in the low percentile is too slight to be significant. Similar results were obtained by comparing the median scores in each level of intelligence. Hence these findings support the conclusions of MacDonald rather than those of Barr and others.

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[3] M. C. Schuyten, Les variations de la force musculaire et le developpement intellectuel des élèves (Reported by A. Binet), *Année Psychol.*, 9, 1902, 448-449, from *Paedologisch Jaarboek*, 1902.

[4] G. M. WHIPPLE, *Manual of Mental and Physical Tests: Simpler Processes*, I, 1914, 106.

[5] M. W. Barr, *Mental Defectives*, 1910, 162.

[6] A. MacDonald, quoted by Whipple, *op. cit.*, 106.

[7] J. E. W. Wallin, *Clinical and Abnormal Psychology*, 1927, 499.

[8] Whipple, *op. cit.*, 101-2.

[9] Wallin, *op. cit.*, 499.

(3) The data were, in the third place, arranged to compare the index of dextrality with the intelligence ranking. The term "index of dextrality" is used by Whipple to indicate "the superiority of one hand (whether right or left) over the other,"[10] and is expressed as a ratio between strength in one hand over the other. This comparison is pertinent because "superiority of one hand over the other is evident when the child enters school, but becomes increasingly evident as maturity approaches."[11] If, then, handedness is related to intelligence, those in the higher percentiles should have a greater diversity than those at the central tendency, and those in the lower percentiles should be more or less ambidextrous.

The findings again showed no significant correlation, the coefficient being only $0.07 \pm .03$. There were, for example, 10 *Ss* in the lowest intelligence level and 12 in the highest who were equally strong in both hands. This illustration is typical of the results.

At the end of the first semester in 1931-1932 a study was made to ascertain whether the instructors' grades were related to the strength of grip in either hand and to the index of dextrality. For this purpose 50 cases were taken at random, and the check merely supported the findings of the more extensive study mentioned above.

It appears, therefore, that there is no significant relation between the two variables —strength of grip and intelligence—among college freshmen. The investigators who have found a correlation have dealt with dull children, delinquents, or mental defectives. It may be that subjects of these types are neurologically predisposed to ambidexterity, but if that be the case and if a relation exists between strength or ambidexterity and intelligence, a sampling of a group of young normal adults should show, at least, some suggestion of it.

Bucknell University PHILIP L. HARRIMAN

SOME OBSERVATIONS ON AFTER-IMAGES OF BODILY MOVEMENT

The observations here given are fortuitous rather than designed, but are, I think, suggestive enough to be worth recording. They concern after-images of bodily movement following several days at sea and are reported from notes made as soon as feasible after the events described.

Sometime ago I had occasion to go on a cruise off the Maine coast. The boat was a thirty-foot sloop, staunch and deep, yet, because of its small size, not free from rolling and bobbing in a seaway. For three and one-half days we were on board, without once setting foot on land. Possibly one-half the time was spent 'outside' (beyond sheltered water), where considerable ground-swell was always present and where, from late forenoon to early evening, it was usually choppy or rough; the rest of the time was spent 'inside,' mostly at anchor, where swell was absent but where the breeze was generally sufficient to roughen the water and produce a clearly perceptible movement of the boat. Accordingly, for the approximately eighty-four hours of the trip our bodies were in almost constant motion.

We returned on the late afternoon of the fourth day. Upon stepping ashore I noticed a lack of steadiness in balance, which lasted for upwards of an hour, but

[10] Whipple, *op. cit.*, 100.
[11] *Ibid.*, 107.

no reactions which could properly be called after-images. About three hours later, however, when the excitement and activity consequent to returning had abated, a striking experience occurred. I was standing in a relatively quiet room, feet slightly spread, quite relaxed and unoccupied, and conscious of general fatigue, when suddenly the floor beneath me began to rise and fall, at the same time rolling and tipping in all directions. This continued for perhaps a minute. The movements were rhythmical, but not uniformly so, and were unmistakably like the rhythmical but irregular movements of the sea. The rise and fall varied from hardly perceptible excursions of a half-inch or so to six or seven inches, and the rolling and tipping varied from less than one to several degrees. The movement was felt rather than seen, and upon closure of my eyes became much more vivid; in fact, there were moments when, with eyes closed, I was sure I was being thrown off balance, although the raising of the lids failed to reveal any appreciable body sway. Consequently with the eyes open the experience was perceived as an illusion, but with the eyes closed it seemed to be real. The cues were distinct changes of pressure in the soles of my feet, and a general kinaesthesis, chiefly from my legs, similar to the kinaesthesis of balancing movements. The experience appeared to die out of its own accord, though I was aware of something like a change of set or attitude, a change of orientation, and I believe this had much to do with the disappearance. There were no recurrences until bedtime.

At that time, upon getting into bed and relaxing, I began to experience the movement with even greater vividness.[1] The bed commenced to rise and fall, roll and tip, and I had definite experiences of changes in cutaneous pressure, as though I were sliding longitudinally a centimeter or so back and forth and rolling slightly from side to side. Had I not known better, in that dark room I could easily have convinced myself that I was still on the boat. There was, again, a consciousness of bodily fatigue. The experience varied in intensity and extent, sometimes momentarily disappearing, to recur, usually fainter, in a manner not unlike negative after-images in vision. I do not know how long this experience lasted, probably not more than a few minutes, for it was lost in the drowsy descent of sleep.

The next morning there was nothing, though I had memory of a dream of being abed on board the boat, which was tossed about considerably by a strong wind and gave me restless anxiety lest it drag anchor and pile up on the shore. During the day there were only one or two occasions when, in a moment of pause, I thought I was aware of slight movement of a fleeting and indefinite nature. Whether considerable occupation with overt matters inhibited the response, or whether it had simply faded I have no way of knowing. Upon retiring that night, however, it reoccurred, but much less vividly than the night before. The next day, and thereafter, there was nothing.

The experience was nearly always pleasurable and lulling; one had, when abed, visions of being gently swayed to sleep.

The following highly tentative generalizations suggest themselves: (1) The movement imagery is like the original movement sensations in quality and pattern. (2) The components are tactual and kinaesthetic. (3) Vision is not contributive;

[1] Dallenbach reports a similar experience after a long automobile ride; cf. K. M. Dallenbach, Recurrent images, this JOURNAL, 35, 1924, 155.

it seems to inhibit. (4) The phenomenon seems to occur only in moments of relative relaxation and pause, and perhaps is conditioned by fatigue. (5) The experience obtrudes; it is apparently not aroused at will. (6) The experience is almost invariably pleasant.

Western Reserve University HERBERT GURNEE

RESPONSES OF ANIMALS TO THE AIRPLANE

In a recent and extensive airplane trip, I noticed (when the plane was flying low enough to permit observation) that the different animals behaved very differently to the stimulus of the plane. Geese, chickens, and other domestic fowl almost invariably fled for shelter as the plane approached. Other and wild birds nearly always took wing and flew away at right angles roughly to our line of flight. The quadrupeds, on the other hand, appeared relatively indifferent. Most of the cattle and sheep were undisturbed. When the plane passed directly over their heads, the cows and horses would sometimes stand rigidly looking up. When we were flying very low, a few cows (not more than one or two in any pasture) would run about excitedly but the alarm did not become general. So far as my observations are concerned, goats and sheep seem to be the most indifferent to the airplane, plow-horses next, and cattle next. Domestic fowl are thrown into a wild panic.

The airplane is essentially a new stimulus. Neither these animals nor their ancestors are likely to have had any direct experiences with it. Reactions to it cannot be explained as conditioned reflexes or as mechanically operative racial habits. The plane does, however, as seen from the ground, bear some resemblance to a large bird. If we assume that animals perceive it as a gigantic hawk, then the differences in their behavior become somewhat more intelligible. Hawks are not dangerous to horses and cattle. These large quadrupeds have no reason to fear birds of any kind. To geese and chickens on the other hand, the hawk is one of the most deadly enemies. If they interpret the airplane as a giant hawk, their frantic efforts to find shelter are not unreasonable.

West Springfield, Mass. MABEL F. MARTIN

THE TWENTY-EIGHTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The twenty-eighth annual meeting of the Southern Society for Philosophy and Psychology was held in Wardman Park Hotel, Washington, D.C., April 14 and 15, 1933.

The Friday morning session, with Knight Dunlap serving as chairman, was devoted entirely to experimental psychology. J. E. Morsh (Gallaudet College), in the first paper, reported comparative data on motor performances from two groups of *Ss* equated as to sex and age, one group being from the Columbia Institution for the Deaf and the other one consisting of persons who were normal in hearing. The performances involved were speed and accuracy of movement, inhibition of movement, hand-eye coördination, speed of eye-movement, and balance. The tests, involving both hands, "were repeated over a period of days to ascertain the effect of practice." Results indicate "the importance of motivation in tests of this kind," and wide variation among the *Ss* in degree of handedness and motor learning. It is hoped that Dr. Morsh will indicate in detail how the differences in motivation operate.

R. H. Peckham (Johns Hopkins University) described a new precision stereoscope for investigating problems in binocular vision, particularly retinal rivalry. The instrument is essentially a four-mirror Wheatstone stereoscope. There are proper lenses for adapting it to the accommodation of the crystalline lens, and there is a vertical background for a projection plane. The background is mounted upon a carriage which bears two telescopes trained on the reflected images of *S*'s eyes. To make permanent objective records, moving picture cameras are substituted for the telescopes. This permits of a high degree of accuracy, and several checks are available. The records of the extent, frequency, and duration of eye-movements or torsions in retinal rivalry, made simultaneously with *S*'s report of his observations, afford "convincing evidence against the drainage theories devised to explain, among other things, temporary functional anopsia."

Frank A. Geldard (University of Virginia) described a case of total color-blindness in a man 23 years old. The data consisted of measurements of acuity, reactions to color and to brightness tests, associated refractive defects, nystagmic movements, and bodily adjustments. An ophthalmological examination revealed findings which checked with those of former studies of total color-blindness.

Sarah Calista Dunlap (Johns Hopkins University) described work in progress on the voluntary modification of the knee-jerk. Improved apparatus is being constructed for experiments on reversing the knee-jerk, on transferring it to the non-stimulated leg. The degree of success thus far attained raises questions as to the methodology of 'conditioning' reflexes and as to the criteria of the 'conditioned' reflex. Great fluctuations were noted, both in the records of any one individual and among those of different individuals.

John E. Winter (University of West Virginia) reported a study of 37 women theft-suspects in which cardio-pneumographs and association tests were compared as to efficiency. It was found that the former records are more 'trustworthy' than association records, indicating the guilty better than the latter method; but such psychograms showed distinct symptoms of guilt in occasional innocent persons. Both methods combined gave more reliable results than either method separately. The guilty persons were determined by confessions, but the experimenter admitted that on the test records alone, without confession, they could probably not have been detected. How does he know that those who confessed were the only guilty ones, even though most of them were probably innocent? At least one of those who confessed hoped thereby to save further embarrassment.

Lyle H. Lanier (Vanderbilt University) reported preliminary studies of the relationship between speed of reaction and certain basic physiological processes. Thirty-five undergraduate men were given speed tests in both simple and discriminative reactions, in two forms of tapping, in marking lines, naming colors, reading names of the same colors, and cancellation. The five slowest and the five most speedy men (as selected on the basis of mean percentile rank scores in these tests) were then measured as to blood pressure and basal metabolic rate, and incidentally also in rate of respiration, pulse rate, and pulse pressure. Though the differences between the two groups in the physiological tests were comparatively slight, the 'quicker' group showed higher basal metabolism rate, higher blood pressure (both systolic and diastolic) and faster pulse rate than the 'slower' group. The latter group had, however, a higher mean respiratory rate. There was no group difference in pulse rate. The technique and the results were regarded by the author as only tentative.

Joseph Peterson (George Peabody College for Teachers) reported analytic studies of the generalizing processes by senior college and graduate students on the Disk Transfer problem. This problem has been used as a learning test in the study of racial differences (see *Ment. Meas. Monog.*, 1929, no. 5, 31 f., and 61 ff.). *S* was required to transfer a set of disks stacked in Circle A (in an order reverse to that of size) to Circle C (in the same order), using Circle B as a 'relay station.' Only one disk must be moved at a time, and no disk is to be put upon one smaller than itself. Errors would make more moves necessary but would not at first be noticed by *S*. Instead of keeping time, which seemed in preliminary work to confuse some of the *Ss*, *E* tallied marks for the several moves, and indicated with appropriate signs the occurrences of certain important errors, as well as *S*'s remarks, his method and special difficulties. For each *S* a demonstration of the solution with three disks was made. *S* was then asked to solve again the 3-disk problem to the point of two trials in succession without any excess moves; next he was required to solve the 4-disk problem; and finally the 5-disk problem. He was informed after each trial as to the number of moves made and the number of necessary moves. After this it was pointed out to him that only one move was sufficient if only one disk was used, that three moves were necessary with two disks, and it was recalled that the 3-disk problem required 7 moves, the 4-disk problem 15 moves, and the 5-disk problem 31 moves. Then he was asked how many moves would be necessary for a 6-disk problem, for a 7-disk problem, an 8-disk problem, and finally an *n*-disk problem. He was asked to go to an adjoining room and work this last one out by developing a formula, taking what time he needed. Results showed marked variations in method of attack, in degree of random (as opposed to rational) procedure, and in moves required at different levels of difficulty. Of 14 *Ss* only 5 supplied the formula and were able to give really helpful general statements supposed to aid a novice beginning the problem. Many *Ss* failed even to keep clearly in mind at critical stages what was to be done next, and showed insight only temporarily here and there. The degree of random procedure was rather surprising.

Two sessions were run simultaneously in the afternoon. One dealt solely with philosophy, and was presided over by Anna Forbes Liddell, the president of the Society; the other was devoted to papers on experimental psychology, Joseph Peterson acting as chairman. The philosophical papers offered were "Pickwickian senses," by Charles A. S. Dwight (Keuka College); "Platonic love," by Adam Alles (St. John's College); "Leibniz' Theory of Knowledge," by Lewis M. Hammond (University of Virginia); "The determinable and its determinates," by John Kuiper (University of Kentucky); "Philosophy and religion," by Lucy Shepard Crawford (Sweet Briar College); "Some historical steps toward parallelism," by Albert G. A. Balz (University of Virginia); "The sophistic attitude in art appreciation," by Marjorie S. Harris (Randolph-Macon Woman's College); and "Frames of reference in philosophy," by W. P. Warren (Furman University).

In the section on experimental psychology, J. M. Porter, Jr. (University of Virginia) led out with a report on studies on 60 epileptics, carried out to determine the total apparent bodily resistance to the passage of a weak electric current, and the galvanic skin reaction (the so-called psychogalvanic reflex), both during 'normal conditions' and during the 'seizure.' Differences were found in the *Ss* between the normal and the seizure conditions only in bodily resistance to the current, the resistance during seizure dropping to a level well below that of normal adults in

general. The total apparent bodily resistance during 'normal' states "was considerably higher than that found in normal adults." Porter held that "the total bodily resistance and the appearance of the galvanic skin reactions are to be regarded as indicative of sympathetic innervation during the seizure in the light of results obtained by other investigators working with preparations in which innervation by the different branches of the nervous system was controlled." Questions arose in the discussion as to whether some of the subjects were not really hysterical epileptics.

Wendell L. Gray (Johns Hopkins University) gave a preliminary report on studies of the effect of forced activity (fatigue) on learning in white rats and on the selection and consumption of food materials. The animals were forced to run various distances in rotating drums and electric shock was employed as motivation. Apparent differences between 'control' and 'experimental' animals were tentatively reported both as to learning rate and as to quantitative and qualitative food consumption.

Wallace B. Varner (Washington and Lee University) reported an experiment on the effects of alcohol on the maze habits of albino rats, designed to test the view of certain pharmacologists "that alcohol affects the nervous system directly." If the hypothesis is true "one would expect that alcohol would give, temporarily at least, some of the same effects found by Lashley in his brain lesion work." Ten albino rats, beginning age about 85 days, were used. Two mazes were used (a simple and a complex one) similar to those devised by Lashley, and the motivation employed was that of getting to food after a deprivation period of 24 hours. The alcohol was not administered until after the maze habits were firmly established. At this stage the animals were subjected to alcohol fumes in a galvanized tank, first for a period of 5 min. each day, and then for progressively longer periods until the animals "were obviously under the influence of the drug." The animals were daily tested in the maze in the order simple–complex–simple–complex after being removed from the fumes. Results showed that the simple maze habits were unaffected by exposures which "reduced the efficiency of performance in the complex maze by greatly increasing the number of errors, lengthening the running time and by producing neuro-muscular incoördination." Progressive resistance to the drug with continued exposure on successive days made the animals in time immune to degrees of exposure originally effective. The conclusions, that alcohol affects the nervous system directly and that "apparently, narcosis does temporarily what a cerebral lesion does more permanently," certainly needs particularization and detailed analysis. There is nothing unusual or unexpected in the results themselves. But the effect of the alcohol is not "direct" in the sense that removal of parts of the brain is. Miles has shown that alcohol tends especially to paralyze the action of coördination of the hind legs of the rat. Were the present results due to such behavior, or to more general incoördination?

Wayne Dennis (University of Virginia) reported results of a survey of data on the order of difficulty of maze blind-alleys, including results from his own experiments. Data from the distribution of more than 2,000 errors collected only from responses in repetitive simple alternation mazes show that in this type of maze, "regardless of the size, conditions of experiment, etc., alley difficulty (total errors) decrease with serial order for at least the first four alleys. Beyond alley 4 or 5, no further decrease with difficulty occurs." It was found by comparison of

4-alley with 10-alley mazes that the former as to relative difficulty are like the first four alleys in the latter, not like the last four alleys. He concluded that the factor determining the order of difficulty of maze alleys is proximity to the entrance and not to the goal. It might be added that only in mental mazes, where spatial factors are eliminated, does a true backward order of elimination from last to first alley show itself, and that the backward elimination is based on a progressively increasing 'learning coefficient' (proportion of expected runs past to runs into blind alleys) from entrance to goal as based on the law of probability (cf. *J. Exper. Psychol.*, 3, 1920, 277 ff.). There are reasons for doubting that "alley difficulty" correlates perfectly with total errors made in the several blind alleys.

Frank A. Pattie, Jr. (Rice Institute) reported a study of social behavior tendencies in 42 chicks which had been hatched and reared in isolation. Normal chicks of equal number were used as controls. Every chick was individually placed in a test-pen 30 min. daily for 6 days, beginning at the age of 4 days. In one wall of the test-pen were two similar windows. Outside one of these windows were two stimulus-chicks of the same age as the tested chick, and outside the other were two white mice. Provision was made for an automatic recording of the time spent by the tested chick before each of these windows. The normal chicks on an average spent 61% of their time "trying to reach the chicks" and 20% "trying to reach the mice." The corresponding percentages of the chicks reared in isolation were 35 and 32. These differences in themselves may not be significant, but a study of the changes in preferences through the successive days shows a gradual shift from nearly twice (41:23) as much time devoted to the chicks the first day to over 44% (27:39) more time given to the mice than to the chicks the last day. A possible interpretation of this shift was tentatively given by Pattie, with due caution. He suggested that it might indicate an innate social tendency as operating on the fourth day of age, but gradually waning later. Certain data were pointed out as not favoring this interpretation. It is to be hoped that more work of this sort will follow, and the reviewer suggests the desirability also of a check experiment in which the mice are removed from behind the one window, so that the stimuli will be chicks versus no live objects. In view of all the linguistic controversies we have had concerning 'instincts,' carefully controlled experimentation is very desirable.

Roy M. Dorcus (Johns Hopkins University) reported data contrary to results found by Griffith, that "rats subjected to continuous rotation for a long period of time were permanently disoriented and tended to run in a circular path depending upon the direction of rotation." Since retardation and acceleration "are the effective stimuli in provoking nystagmus," Dorcus sought to determine whether the intermittent stimuli of acceleration or retardation would produce any disorientation. His results were negative. There is obviously a need of reducing the differences thus reported from the two laboratories by further experimentation, with most careful statements of details as to procedures and results.

Franklin Fearing and O. H. Mowrer (Northwestern University) reported an experiment on two groups of 10 adult pigeons in each group, designed to determine whether the well-known reduction in vestibular nystagmus by repeated elicitation is due to specific injury to the vestibular receptors, or whether, according to another theory, the reduction is simply a matter of 'learning' or adaptation to a new sort of environment. The pigeons of both groups were given "a standard rotation test for vestibular nystagmus," but the birds of the experimental group were deeply

anesthetized with appropriate injections of sodium ethyl-methyl-butyl-barbiturate (prepared by Abbott and sold under the trade-name of "Nembural") on the 1st, 4th, 7th, 10th, and 13th days following this test "and while anesthetized they were subjected to an intensive program of repeated bodily rotation. On the 16th day after the original test they were retested for nystagmus in a normal condition. Results showed no detectable reduction in their 'normal' nystagmus, while the control group, not anesthetized, showed an average reduction of nystagmus of "over 50%." Suitable control tests showed that the difference "was not due to artifacts of any kind." The authors conclude that the reduction of nystagmus is due to 'learning' or to some analogous process in the central nervous system. The conclusion was questioned in the discussion of the paper, as it seemed to involve an interpretation based on a particular view. It might well be postponed until after some careful supplementary studies from a neurological approach as well as a histological post mortem study of the vestibular apparatus of the rotated birds, shall have been made.

After the annual banquet, the president of the Society, Anna Forbes Liddell (Florida State College for Women), representing the philosophers, appropriately interpreted Cusanus in modern terms to the Society, largely psychologists, in a paper on "Instructed Ignorance, The Philosophy of Nicholas of Cusa." The address will be published in full.

In the Saturday morning session, with Dr. Liddell as chairman, the program consisted largely of philosophical papers, some of which cannot be profitably represented in this short report. B. C. Holtzclaw (University of Richmond) gave the first paper on "Ideas of value in ancient philosophy." P. F. Finner (Florida State College for Women), in a critique of recent literature on 'suggestion,' held that suggestion is not merely a social process but may occur with inanimate stimuli; that it is not to be limited only to direct and immediate responses to stimuli; that it is to be distinguished from the more permanent effects of moods, purposes, and wishes; and that the explanation of suggestion must be found either in an arousal or a change in some neuro-muscular set "that affects responses other than the set itself," or in the arousal of responses through conditioned stimuli. He distinguished two processes properly to be designated by the term 'suggestion.' It is hoped that these may be published for careful examination by psychologists, but meanwhile the discussion in general seemed unfavorable to the use of 'suggestion' in psychology as a technical term, because of its indefiniteness and its implication of forces in conditions that would require more analytic treatment for the determination of the real stimuli.

Floyd C. Dockeray (Ohio State University) discussed emotion as "disorganized response," contending that emotion may be regarded as the opposite of attentive or organized behavior; that it is really a disorganization or maladjustment of the individual's total reaction to a situation. "There are segments of the total behavior which are highly organized, such as certain glandular or visceral reactions; but the total picture is that of disorganization." He held that the emergency reactions described by Cannon "are undoubtedly found to occur in organized behavior as well," and promised supporting data from investigations now in progress. The presentation seemed to give the impression, as is evident in this criticism of Cannon's descriptions, of *a priori* assumptions of the points to be investigated. But we shall await the promised data. Dockeray's incidental admission that he had probably ap-

proached the problem at first from a too "mentalistic" position brought criticism of the implied dualism not only in this term but also in the implications of terms like "behaviorism," "purpose," etc. Direct explanations of activities in terms of known factors, or of testable assumptions of the operations of such factors, seemed to be preferred by those who discussed the paper. Reactions which phylogenetically may have preserved animals possessed of certain mechanisms with emergency value may of course seem to be 'disorganized' from the standpoint of what the individual was doing before the emergency arose. The response of a newly decapitated hen could hardly be cited as an ideal case of emotional reaction.

John E. Bentley (American University) discussed relations between intelligence and intellect.

H. M. Johnson (American University), discussing epistemological realism with respect to the question as to whether sensory content is public, took the position that even though such realism presupposes that sensible qualities (sensibilia, sentienda) exist independently of their perceiver, and therefore are not changed by reason of being perceived, this does not imply that the sensory content of one individual can be detected as such by another. He criticized the realists who deny the privacy of sensory content, as falsely identifying *sensible* qualities with *sensed* qualities. He held that an indeterminate number of sensibilia may occupy the same 'locus' of public space at the same time, so that even though two individuals may at any given instant be pointing to some spot in which they see what they agree in calling *red,* they may be pointing also to other sensibilia than red. The only way either one may indicate to the other which of the compressed sensibilia he is perceiving is to exhibit his collection of *sensed* qualities. However, as Bertrand Russell and others have pointed out, the system of reference of the one person's percept is so correlated with that of the other person's percept that, although the terms of the relationship, namely the *sensa,* remain private, the interrelations are public.

H. C. Sanborn (Vanderbilt University) presented a paper on the views of Hugo Dingler as expressed in his 1928 publication, *Das Experiment.* The paper aroused much discussion.

The Saturday afternoon program, with Buford Johnson as chairman, was devoted largely to child psychology. Maurice C. Langhorne (Emory University) described an experiment on 78 children, of ages ranging from 7–17 yr., in the training to operate the Renshaw-Weiss pursuitmeter. Sexes were about equally represented. His work was designed to evaluate the truth of two alternative hypotheses set up by him on the influence of the age of the learner on the acquisition of skill. The hypotheses were stated as follows: "If the skilled act or habit is formed by a reorganization of old order habits, then it may be argued that older *S*s, with a fuller complement of habits, should acquire skill more rapidly. If, on the contrary, the process of habit formation does not involve the mere revision of neuromuscular systems already present, but is an acquisition *de novo* of sensory-motor controls, then the rate of learning should correlate inversely with life age." Each child ran 36 cycles, 4 min. each, during 18 consecutive days. Results show that both the limit and the rate of improvement by practice increased directly (but not in parallel with each other) with age, with the maximum "acceleration in improvement" occurring at about 13 or 14 yr. of age. Boys were superior to girls. We can hardly agree with the author that the first of his two hypotheses is confirmed by these results. It

is probable that neither of the two hypotheses is correct, that there is no such sharp line to be drawn between "organization of old order habits" and "acquisition *de novo* of sensory-motor controls." However, certain practical inferences as to the relatively favorable periods of age to begin the training in certain types of skill acquisition *may* be made. Such inferences will of course have no relation to the main problem of the experiment.

Grace O. McGeoch (Stephens College) reported that an analysis of the experimental literature on the whole-part problem, including an experiment of her own, reveals no inherent superiority in either method *per se,* their relative efficiencies in any experiment being a function of a large number of uncontrolled irrelevant factors. A further study in 172 9-, 10-, and 11-yr. old children, in which the *S*s had been allowed to find out their own preferred methods, showed that 94 of them used the 'verse-by-verse method,' 69 the 'line-by-line method,' and 9 the 'whole' method. No reliable differences were found between the first two of these methods. On the bases of these results she called in question the "customary textbook treatment of ascribing superiority to the whole method." There is, unfortunately, still room for further controversy on this old theoretical problem, inasmuch as its solution experimentally involves control of so many (probably uncontrollable) factors as to make solution practically impossible.

John A. McGeoch (University of Missouri) discussed and presented data on the problem as to whether interpolated learning is more effective in producing a decrement in the reproduction of given memorized content when introduced immediately after the original learning than it is when introduced at the end of the intervening retention period just before the relearning of the content. In case the former is true the results would, according to one view held since the work of Müller and Pilzecker, be regarded as favoring the perseveration theory. It is a question of whether interference (the so-called retroactive inhibition) is greater when the interpolated learning is introduced soon after the original learning or later, just before the relearning begins. The experiments were carried out to test this question. In one of them, on the retention of lists of words, the delayed interpolated learning was introduced just before relearning, after periods of 30 and 60 min., and 24, 48, and 144 hr. In the other experiment, on stylus-maze learning, the delayed interpolated learning came just prior to relearning after a period of 24 hr. In each of these experiments, which were on college students, both the immediate and the delayed interpolated learning of different materials brought about a decrement of the relearning of the original material; but there was either no reliable difference between the immediate and the delayed interpolation or the latter showed the greater degree of interference, in contradiction to the perseveration hypothesis. It is possible that any supposed difference between retention by perseveration and otherwise needs investigation in the light of our present better knowledge of the neural processes involved. Possibly the hypothesis under investigation has no real meaning. At any rate, the problem of the effect on reproduction of different degrees of delay in the interpolated learning has certain values irrespective of its bearing on perseveration.

J. E. Bathurst (Birmingham-Southern College) reported results of an experiment on sympathy and resistance (negativism) in 36 boys and 37 girls of ages between 1.5 and 5.5 yr. He had measured 'sympathy' by telling a story designed to elicit sympathetic responses, and 'resistance' by encouraging procedures involving

"actual contact" with the children when sympathetic responses were not elicited. The child's environment, his possession of pets, age, and sex were considered in relation to the sympathy and resistance scores assigned. While girls showed slightly more resistance than boys, the author did not remark that both *E* and the 'hero' of his story, told to evoke sympathy, were males, as developed in the discussion; but girls and boys did not differ reliably in sympathy responses. The possession of pets was found to increase sympathy significantly, although other environmental factors did not affect either sympathy or resistance. Sympathy in both sexes increased with age. Unfortunately the score points were assigned arbitrarily and were not based on any objective standardization experiments.

W. C. Beasley (Johns Hopkins University) made a preliminary statement of work in progress on problems regarding the vision of newborn infants. "Numerous traditional views have been found to be grossly in error." While a survey on the ocular functions of 250 newborn infants at the Woman's Clinic of the Johns Hopkins Hospital revealed important individual differences, evidences of "fairly acute" visual discrimination was found in all of these infants by the tenth day, and earlier in most cases. Pupillary reflexes develop in 5–8 days; and horizontal, vertical, and circular pursuit responses improve markedly during the first 10 days. Squints, or incoördinate binocular fixation during pursuit, are infrequent and not typical, while coördinate fixation can be elicited in a considerable distance range during this period. A special technique for investigating visual acuity and color vision has been developed. It is to be hoped that with improvement in such techniques, early studies of the relations of visual, auditory, and kinesthetic localizations and interrelations may be followed closely in their relations to the growth of activity.

Beverly von Haller Gilmer (University of Virginia) reported an analytic study of spontaneous responses in newborn infants through the tenth day, dealing both with data from cinematographic records and with observational data. Responses like crying, stretching, sneezing, mouthing, and other such isolable movements are found to be patterned into body-wide organization, supporting the view "that mass activity is not the only total bodily response of infants."

In the final paper of the meeting, Martha Vance Ellesor (University of Virginia) described an experiment to test "the hypothesis that visual stimuli arouse crying only through a process of conditioning." Two children—ages are unfortunately not given—were individually stimulated with the following: a devil's mask, a flashlight, a brown fur, and a movie. Each stimulus was repeated "on successive days until the child lost interest in that particular object." On the first presentation crying was the response elicited by every stimulus but the flashlight. On repetition of the several stimuli crying and negative responses gradually gave way to responses of "investigation and examination" followed later by smiling and laughter, before interest was lost in the objects. The conclusion, that not all responses to visual stimuli are acquired by conditioning, is not surprising. A greater variety of stimuli tried on a considerable number of children with known developmental backgrounds and studied from an early age with changes of stimuli is desirable, as well as an objective method of recording responses so that individual interpretation may not replace detailed exhibits of actual behavior. The work is obviously regarded by the author as only preliminary.

At the business meeting, following the Saturday morning program, Roy M. Dorcus of Johns Hopkins University was elected president of the Society for the next year, and Knight Dunlap and Frank A. Geldard, were elected members of the Council. The Committee on the Teaching of Psychology, now consisting of 20 members, with P. F. Finner, Florida State College for Women, Tallahassee, Florida, as chairman, submitted a tentative report which gives the institutions within the Society's field that are qualified (a) to teach only elementary courses in psychology, and (b) to teach and train in both elementary and advanced psychological work. The report also lists, with an asterisk, all instructors qualified for teaching only elementary courses in psychology. These data were submitted with requests for supplementation and correction by members who notice deficiencies or errors, so that the final report may be as accurate as possible. This committee also distributed a mimeographed list of 10 American psychological journals and of 430 books printed in English on psychological and related topics. By some slip of the 'censor,' it appears, one book in a foreign language was also included—Fechner's *Elemente der Psychophysik*. The list represents books taken from three different sources: those marked "D" being on "the list prepared by Dr. Dockery;" those marked "A" being on "the list of the American Library Association;" and those marked "S" being books "that the majority of members of the Southern Society for Philosophy and Psychology who checked the list approved." The list is apparently also in a beginning stage of development. It seems important that it should be carefully gone over by, and suggestions obtained from, psychologists widely distributed over the country. It is intended, apparently, although there is no statement to that effect, as a sort of 'minimum reading requirements' suggestion for smaller institutions. It will be beneficial if it does not, in other institutions during times like these, check somewhat the efforts to secure recent valuable books and magazines from foreign countries. Since the War the teaching of German, for instance, has suffered serious handicaps, and even yet it comes back into the high schools with much difficulty.

It was voted to accept the invitation of Birmingham-Southern College and to hold the next meeting of the Society in Birmingham, Alabama, March 30th and 31st, 1934.

Peabody College JOSEPH PETERSON

THE PHILADELPHIA MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The twenty-ninth annual meeting of experimental psychologists was held at the University of Pennsylvania on April 12-13, 1933, under the chairmanship of Professor S. W. Fernberger. The meetings were attended by eighteen members from thirteen laboratories. After a short business session, reports of the work in progress were received from the following universities and colleges: American, Brown, Clark, Columbia, Cornell, Harvard, Illinois, North Carolina, Peabody, Pennsylvania, Princeton, Vassar, and Yale.

The next annual meeting will be held at Harvard University on Tuesday and Wednesday, April 3-4, 1934, under the chairmanship of Professor E. G. Boring.

K.M.D.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Animal Drive and the Learning Process: An Essay toward Radical Empiricism. Vol. I. By EDWIN B. HOLT. Supplementary Essay on The Material World. By Harold Chapman Brown. New York, Henry Holt & Co., 1931. Pp. xi, 307.

'Radical empiricism' (from William James) is described as "a way of thinking which aims to escape . . . from the absurdities of subjectivism and any form of psycho-physical parallelism." The author points out that there are two current explanations of motivation or of answers to the question, "What is it that moves an animal to action?" The one points to chemical energy derived from food and from various sorts of stimuli, while the other ascribes the action to feeling, emotion, images, desires, or even to the older faculties, using a mere name for the explanation. This latter explanation he calls 'word magic.' Conation, will, fiat, instinct, purpose, insight, or even *Gestalt,* are examples in human psychology. Holt seeks to avoid both this word magic and controversies, and to make explanations "in terms wholly of physical and physiological processes" (p. 7).

Growth is not a mere "unfolding from within," but a developing life process constantly assisted from such environmental factors as warmth, moisture, specific nutritive substances, and in some cases light. The constancy of pattern in development, attributed by many writers to "innate conditions," depends on constancy of the external environment, and "the phenomena of structure and function are, in living organisms, never independent of each other" (p. 12). The work of C. M. Child is considerably drawn upon. Holt contrasts the older conception of 'growth,' as an unfolding from within, with 'learning,' as a process brought about by effects of the environment, only to insist that the processes are not so distinct; that the *'structure'* of the organism, the organic pattern, is in considerable part acquired through the influences of external stimuli which produce excitations that are transmitted through the chemically formed but organismically unformed tissue of the embryo. In spite of his insistence upon the rather extreme effect of external factors in the development of organic structures, he holds that "all anabolic processes are doubtless carried on and regulated by the *enzymes* contained in the fertilized egg" and are probably "the sole basis, in any true sense, of heredity" (p. 17). These enzymes *select* from the food material available, "and they determine *how* this shall be synthesized into higher organic compounds (anabolism) to make the *substance* of the organism. But this raw substance acquires ('learns') organic pattern by an inverse process," katabolism. Both of these processes go on together.

Holt regards it as established that the "primitive neuroblasts develop into neurones by growing out along axial gradients," contrary to the preformist's view of inherited sensori-motor connections. These questions are now matters of observation; the growing neuroblasts, in making their way through the embryonic tissue, look much like the growing roots of plants which turn from light toward moisture, select certain chemical substances and avoid others. Both the axones and the dendrites of growing neuroblasts follow electrical gradients, the dendrites growing *toward* (Kappers' law) active neurones or nerve bundles (kathode pole), and the axones

away from them (Bok's law). According to Bok, when a bundle of unmedulated nerve-fibers grows out and passes neuroblasts, these latter become activated by it and send forth axones in a direction perpendicular to the activating bundle, apparently taking the same direction as the current which irradiates sidewise from the growing bundle. Even the cell body may migrate in the direction of the dendrites. These laws of neurobiotaxis, according to Holt, who in this regard follows Kappers, are, with the small additional consideration as to the direction in which the excitations flow, identical with "the psychological law of association" (p. 21).

Holt goes on to show "that *no* independent new principle enters in to interrupt this continuity of development including 'psychological association' even in the adult stages of life, and that this same developmental process in its later phases is mentation and consciousness" (p. 21). An attempt is then made in Chapter 5—by noting the valve-like or one-way action of the synapse, the probable tendency of every neurone of the motor side of the central nervous system to spread its receptive areas (dendrites) toward the terminal arborizations over which the impulses that are to be conditioned are arriving and thus to extend the receptive areas of the dendrites—to connect these known facts with the processes involved in Pavlov's conditioned reflex, and to account on this basis for the entire mental growth ontologically. Holt thinks "that the only *original* 'unconditioned' impulses are such as make their way through the central nervous system and out to the muscles *at random;* corresponding to the picture of general random movements in the foetus" (p. 27). His opinion is that "this growth of dendrites under the stimulus of nerve impulses is the sole basis of learning" (p. 27). In general, then, any organism is actually propelled, but not guided, by the food energy stored in its tissues, especially its muscular tissues; but it is guided by external stimuli of different kinds operating through the sense organs, by means of dendritic conditionings in the central nervous system as suggested in this chapter.

Several weeks prior to birth mammals show a markedly random type of behavior; it is so uncoördinated that a touch on the eyeball, for instance, will produce a response of almost any organ as readily as of any other. It is generally thought that such synaptic connections between neurones as are necessary for coördinated movements have not at that stage of development been made. In the human infant such random movements are rather frequent even after birth, due to various sorts of inner and outer stimulation. These first utterly random and somewhat jerky movements "become biologically useful, significant, or 'purposive' only very slowly," some of them (especially when new situations are met) persisting even throughout life (p. 31). But even in the late foetal period more definitely coördinated and regular movements, presumably due to the development of synaptic connections, make their appearance. Whether or not these increases in coördination with the advance in age are due to nerve-fiber growth along physiological gradients, or to effects of practice in the reduction of resistances, the statement that "every passage of a nervous impulse across the junctional tissue between two neurones (at the synapse) lowers the resistance of that tissue to the passage of all subsequent nervous impulses" (p. 34) is now subject to criticism on the basis of considerable experimental data.

The effect, then, of conditioning in the central motor dendrites, Holt, following Bok, holds is to produce 'circular reflexes' through the operation of the propriocep-

tive impulses aroused by the contraction of any responding muscles. Any incoming excitation "will find outlet along the tract just used by the random impulses, that is, will go back to, and will further contract, the very muscle from which it came" (p. 38). In the course of time all the muscles will be involved, "and in all of them such a reflex-circle will then be established. The result is that in every muscle a contraction tends to reënforce and to perpetuate itself" (p. 38). But any response may also produce other external stimuli. For example, if a random impulse reaches the flexor muscle of a finger the response of the finger will cause it to press on the palm. The afferent impulses from this contact will, when this "has happened a few times (as it is bound to happen)," effect a reflex-circle so that a pressure stimulus on either finger or palm will bring about the 'grasping reflex.' This reflex is thus established in the foetal period. Infants "born deaf do not learn to speak even though their vocal organs be absolutely normal" (p. 39). While this statement, from Preyer, is offered as evidence of the general circular-reflex principle, borrowed from Sherrington, the facts are obviously interpretable just as well in other ways which also do not imply "inherited engrams" for specific speech forms. Using language is something more than a mere reflex response of the vocal organs, and to bring such responses and other complexly coördinated acts under the simple principle of a conditioned circular-reflex is certainly a bold hypothesis, one, however, that should not be hard to test out experimentally on young children and animals. Why, for example, in normal children, should "a sound received at the ears cause the vocal organs reflexly to reproduce that sound as closely as their anatomical structure permits?" This is not at all clear in Holt's explanation of the acquirement of speech. Such a basic assumption cannot safely go uninvestigated.

Certainly, without selective reactions by other persons in the social environment, the child would never thus respond. In Holt's explanation he adopts the word "adient" response (suggested by H. C. Warren) as one giving the organism "more of the stimulus" which elicits the response, and "abient" response as one giving "less of the exciting stimulus." Whether every reflex-circle response will cause the organism to get more of the exciting stimulus, as Holt holds against Bok, or whether it will cause it to get less of this stimulus seems to me to depend on whether this response brings about other exteroceptive stimuli practically simultaneously with the response and only during or immediately following its recurrence, or whether it removes the organism from these stimuli. Only in the former case (see *Psychol. Rev.*, 33, 1926, 218-236) can the incoming impulses find outlet in simultaneously active motor channels (physiological gradients or nerve fibers). I see no other reason why the incoming impulses from a given stimulus, or even the proprioceptive impulses, otherwise should go out to the particular muscles that responded to the next preceding stimulus. What is the specific evidence? How can learning ever take direction if stimuli merely effect circular-reflex responses?

Holt seems partially to recognize this deficiency of his circular-reflex response position, where in a note (p. 42) he says: "By avoidance I mean immediate retraction from the stimulus so that the stimulation ceases." I see no need then for the circular-reflex principle at all, and Coghill's work hardly supports it, even though it is cited in this same note. Holt himself seems to have misgivings (cf. p. 43). The next chapter, on "Circular Reflexes and Progression: Inhibition," runs

further into difficulties of a technical nature which would require too much space to examine in detail. Holt traces the probable expansion of coördinations from the mere (supposedly) circular-reflex type to hip, shoulder, and limb muscles, flexion and extension movements of the same limb, and correlations between fore and hind limbs, front and back limbs, and among all four limbs in certain animal forms. He then goes on to consider how the organism, on the same principles, becomes responsive by direct and indirect stimulation to "the pull of gravitation," *i.e.* how postural reflexes are developed. Here the proprioceptors continue to play a very large and increasing rôle. It is obvious that any movement of the limbs or head immediately effects changes in the contractions of different muscle systems and preparatory sets for further changes. All these postural tonic reactions are regarded as "purely reflex," not being impaired in decorticated animals; and according to Magnus they are dependent only on the red nucleus of the mid-brain, as the highest center, and do not even involve the cerebellum (p. 70). Yet several of these responses to gravitational conditions are so well coördinated as to involve all-or-none responses of elementary units working on the principle of interrupted responses (refractory phase).

Along with the developments in motor coördination goes "education of sensory surfaces," which Holt infers to be due to the gradual inhibitions or facilitations of impulses of opposing or of coördinating tendencies, until the several sensory points of stimulation coöperate. Here again there is danger of over-simplification in terms of mere discrete reflexes.

Recent results obtained by Beritoff (1926) and by Switzer (1930) show that a stimulus coming slightly after a response may get a control over it, though the conditioning is more difficult than if the stimulus precedes the response. Moreover, there are some indications that the temporal disparity between stimulus and response to be associated may be not merely a few seconds, but even 30 or more minutes (Pavlov). Holt sees two possibilities as indicated by these data: "The first is that a nerve current, after it has passed, leaves behind, at synaptic regions, a temporary condition which is analogous to a state of tension or 'charge'" (p. 88). The second possibility is that "an uninhibited ***circular-reflex*** (perhaps of low intensity) may be operative. At any rate, he sees in such hold-over effects of nerve impulses possibilities that ***"a total afferent pattern*** (not merely one or two afferent currents), may be effective in producing the next response, as hitting the next 'right' chord on a piano, or singing the next note, to continue the sequence of a musical selection that one has begun. Holt does not, of course, overlook other motor connections, though he regards them as not sufficient; and he also recognizes the numerous stimulations constantly affecting us from the general surroundings. Chain reflexes are set up between successive reflex responses, when the resistance becomes lowered (canalized), so that the final act of one functional unit in the series excites (directly or indirectly) the next. "Deep and early canalization" Holt maintains, is one of the two essential conditions that determine which shall be, in the life of the individual, the prepotent reflexes, or the (acquired) 'instincts.' Examples are the peristalsis of the gullet and intestines, relatively isolated from extraneous motor channels.

Many important details must be omitted here. In general, Holt describes four factors which make for integration: "first, the external reference of all learned re-

sponses: second, the mutual summation and inhibition of responses; third, simultaneously sustained responses with a residual locus of freedom (for random movements); and fourth, cross-conditioning" (p. 230). These are not to be regarded as wholly separate or coördinate factors. The second is particularly questioned in this regard. In the case of the third, the author holds that every response that is effective simultaneously with others—"whether tonic, postural, or actively phasic" —narrows the locus of freedom for random movements, thus making the action more definite and precise. Cross-conditioning is the reënforcement or canalization of certain overt activities by their association with or attachment to basic and necessary bodily rhythms—heart and lung activity and other autonomic processes—so that they become rather permanent tendencies or settings of the organism's motor predilections and general character. These predilections thus come to serve as "ready and characteristic paths of nervous 'overflow' in every situation of partially obstructed activity" (p. 226). A fifth possible integrative factor is 'motor block,' which "is closely related to inhibition" (p. 230). When any obstacle to certain motor activity is encountered there seems to be an overflow of nerve impulses into other closely related channels (though our ignorance is still too great to specify or to be certain). Various random activities, often not very accurately anticipated even at this advanced stage of learning, result. Some of these supply a way out of the difficulty and the organism moves on in accordance with its own inner drives and external, guiding stimuli. Self-interest, the constant movement away from annoying and blocking stimuli, is always the rule. Therefore "an appearance of 'end sought' always characterizes . . . the process of learning by random trial. This appearance has given rise to the much misleading speculation about a *vis a fronte*. In reality the *vis* is always strictly *a tergo*" (p. 239). "That it [behavior] should *all* be in the last analysis self-interested, follows inevitably from the physiological structure of every organism" (p. 240). The scheme is acknowledged to be but a very rough sketch of enormous complexities which can be filled out in detail only when our present knowledge shall have been greatly extended. Holt appropriately criticizes vague notions of the 'organism as a whole,' and denies that the organism ever does act as a whole. "To say that a 'whole' regulates or in any wise operates on its parts makes the 'whole' something other than the totality of its interrelated parts, a metaphysical other that lends itself to viscious thinking."

The book is so stimulating and vigorous in its constructive features that psychologists will await with interest the appearance of the promised second volume. There are fifteen pages of "works cited." The essay on "This material World," by Harold Chapman Brown, is reprinted from *The Journal of Philosophy*, 22, 1925, 197-214.

J.P.

Studies in the Dynamics of Behavior. By CALVIN P. STONE, CHESTER W. DARROW, LENA L. HEATH, and CARNEY LANDIS. Edited by Karl S. Lashley. Introduction by the editor. Chicago, University of Chicago Press, 1932. Pp. xiv, 332.

Three separate studies on the dynamics of behavior are reported in this volume. The first, by Stone, is on "Wildness and Savageness of Rats of Different Strains."

Stone used, in this study, five different kinds of rats between 3 or 4 mo. of age: wild trapped rats, pure albinos, crosses between the wild and the albinos, and

between the half-breeds and albinos, and a group of yellow hooded rats. In his first experiment he attempted a quantitative estimate of wildness and savageness. In mean ratings, the groups arrange themselves in a descending order as follows: full-wilds, half-breeds, quarter-breeds, yellow hoods, and pure albinos. Differences in group variability are small. A similar quantitative study of younger rats should be made so that the effect of training on these activities might be measured, the pattern of the behavior studied, and the degree to which experience determines these patterns noted. While the effects of experience are hard to eliminate entirely, they are reduced in this study to a minimum. An interesting check on the results would be to use albino rats that had been allowed to grow up wild from birth. This should give some clue as to the stability of the change in the direction of tameness which has been produced in albinos by breeding and training.

To test the dominance of the hiding tendency over hunger, apparatus consisting of three lengths of stove pipe arranged in a rectangular form with a starting box at one end and a food box at the other was used. The hiding tendency was measured by the time spent by the rat in the stove pipe. In descending order of strength of the hiding tendency the groups ranked as follows: full-wilds, half-breeds, yellow hoods, quarter-breeds, and albinos. The author does not state or infer that this hiding tendency is inherited, but seems to associate it with the other forms of behavior characteristic of the wild group. How would this tendency in the albinos be influenced by living a wild life?

In the third task set the animals, the learning of a simple maze, it was found that the wild parent stock could not be induced to learn the problem. However, in terms of time and error scores the half-breeds and quarter-breeds were superior to the albino and yellow-hooded groups. Though no explanation of this result was found, Stone concludes that by systematic handling and strong hunger motivation would probably produce a degree of persistence and effort in genetically wild rats comparable to that found in the domesticated albinos.

In the second investigation, Darrow and Heath obtained measures of physiological changes elicited under standardized conditions and tried to check these measures against scores on the Thurstone Neurotic Inventory, on the Northwestern Introversion-Extroversion Test, and on certain available scholarship and intelligence ratings. Under laboratory conditions stimuli were presented to 90 college students to elicit "behavior of a psychological significance, such as anticipation or anxiety, relief, adaptation to repeated stimuli, summation of responses to repeated stimuli, conditioning to indifferent stimuli, and rate of extinction of the conditioned responses." Measurements were made of arm movement, blood pressure, respiration and the galvanic skin reflex. The stimuli employed were a standardized electric shock, verbal warning of shock, announcement of a rest period with no shock stimuli, and clicks of the pendulum used in governing the time of the electric shocks. The strength of the shock to each individual was so adjusted as greatly to reduce the variable effects usually obtained with a constant strength for all subjects. The shock was standardized in terms of the strength of a tetanizing current just strong enough to effect the flexion of the thumb.

Twenty-one different types of measure, both absolute and relative, of changes occurring during the stimulating situation were correlated with the questionary scores and with separate items within the questionary.

To increase the reliabilities, empirical groupings of personality items and of physiological reaction-tendencies were made, depending upon the relationships intrinsically found in the data. Ten different "personality constellations" were thus formed and separate scores for each computed. These were correlated with the standard scores of the physiological measures. "Physiological syndromes" of reaction-tendencies were then identified for each of the personality constellations. Correlations between physiological reactions and composite personality scores were generally low, only 14 of the 240 being above 0.20. The five highest coefficients were between 0.26 and 0.31. The authors conclude that their physiological data show no temperamental types, as determined by the questionnaires. The coefficients between the physiological syndromes and the personality constellations were again very low, the highest ranging between 0.30 and 0.38. It is acknowledged that from the point of view of predictive efficiency such coefficients are worthless.

The authors offer an important criticism concerning the measures used to indicate changes in resistance, which is applicable to the majority of studies on the galvanic reflex. They point out the need of a continuous record of resistance and the use of relative or percentage resistance change rather than absolute resistance change as a measure of the reflex. *Their argument finds support in the results of other investigations, and, if valid, reduces to a meaningless hodge-podge the recorded measurements of the majority of investigations on the galvanic reflex.* (Cf. *supra* 502ff.; 522ff.)

The third study is an effort to make a comparative evaluation of the various methods which have been used to measure emotion, emotional stability, and emotional psychopathy. The first part of the investigation is concerned with "a study of 100 delinquent boys" and the second with "a study of 112 delinquent girls." The following measures were used with both boys and girls: Thurstone's Personality Schedule, a test of humor, and an X-O test. In addition to these the boys were tested on a pursuit-meter and stabiliometer during which test measures of the galvanic reflex were made. They were also given Allport's Ascendancy-Submission Reaction tests, and their educational quotients were computed from grade-placement records. The girls were given Heidbreder's Introversion and Extroversion Test, Hull's Test of Suggestibility, and a maze test. Institutional records giving incidents of social history were available for both groups. Juvenile court records were available for some of the boys. Measures of temperature, humidity, and barometric pressure were obtained for the days during which the boys were tested on the pursuitmeter. Since most of the tests were constructed for use with college students the authors endeavored to adapt them in different ways to the delinquent groups.

The general conclusion to be made from the study of these two groups is that the measures used, in every particular, fail to show any diagnostic value in reference to emotional stability or emotional psychopathy. No relation was found between age, race, or social offense and emotional stability, or between scores on any parts of the various tests employed. All tests of emotionality failed to give diagnostic results when used with the juvenile delinquents, even those tests which had been especially adapted for these groups. Out of 134 intercorrelations obtained from the group of boys only, 10 were above 0.30 and of these only two were greater than 0.40.

The author, in view of some of his former investigations on the rôle of such

factors as blood pressure, respiration rate, etc., in emotions, attempted particularly to determine the diagnostic value of the galvanic reflex. Much has been written of late on the use of this reflex as a sign of emotion. The general logic of the argument has been about as follows: (1) the autonomic nervous system, especially the sympathetic system, functions in the type of behavior called emotional; (2) the galvanic reflex is dependent upon the integrity of the motor fibers of the sympathetic system and is disturbed when the supposed nervous center of this system in the thalamus is injured, and therefore it can be used as a sure sign of the activity of the sympathetic system; (3) the galvanic reflex can then be used as an index of the emotional reactivity of the individual in which it is found. Landis' results are quite opposed to this whole line of reasoning. He found (1) no galvanic reflex invariably accompanying the objective signs of emotional disturbances; (2) no significant correlation between either the galvanic reflex and tests of emotion or this reflex and personality as judged from the previous records of the individuals; (3) no unequivocal evidence of a relationship between the reflex and the activity of the autonomic nervous system. In accordance with his formerly expressed views he concludes that the galvanic reflex is just another physiological change such as blood pressure, respiration, etc., which may or may not occur together with one or another type of psychological activity such as emotion or attention. The argument that the galvanic reflex is specific to emotion is thus not substantiated by experimental fact. Due to the present wide use of this reflex in studies of emotions his findings are both pertinent and important. The whole problem of the functional significance of the autonomic system in emotional responses should be reopened and further experimental investigations performed on the first two arguments above. Such investigations, however, will probably have to await the time that psychologists can agree on a set of criteria by which emotional responses can be differentiated. Valuable parts of the third study are the records of the individuals of both groups on the various tests used together with certain data from their social histories. These records are in the two appendices.

University of California C. W. BROWN

Economy and Technique of Learning. By W. F. BOOK. New York, D. C. Heath & Co., 1932. Pp. vii, 534.

The author states in his introduction that the volume is intended primarily "to serve as a text for teacher-training courses in colleges and in schools or departments of education" Employed in the light of this aim, it should serve to give the student in such courses a stimulating introduction to some of the more widely recognized problems of learning and should prove of genuine value to those whose knowledge of the subject is confined to an elementary course in psychology (which the author advises as a prerequisite to its perusal).

Having defined learning as "the process of acquiring the ability to do something that the learner never did before" (p. 16), Book proposes (p. 21) as essential phases or conditions of the learning process: (1) a motivating condition within the learner that makes him react; (2) a stimulus toward which the learner reacts; (3) an appropriate reaction to the stimulus; (4) some consequent change or modification produced in the organism as a result of the stimulation and the responses thereto; and (5) a check on the learner's responses. Learning may occur, we are informed, on

a variety of levels, among which the following are distinguished: (1) racial learning—evolution; (2) physiological learning—*e.g.* adaptation to bad air; (3) conditioning; (4) perceptual learning; (5) representative learning (thinking); and (6) learning how to learn.

Book postulates six "elements of learning" to each of which a chapter is devoted. (1) Element A: Strengthening of existing connections by exercise. (2) Element B: Weakening of existent connections by additional learning, inhibition or disuse. (3) Element C: Establishing of new linkages between new stimuli and old responses. (4) Element D: Linking of new responses to old stimuli. (5) Element E: Invention of new responses and linkage of them with old stimuli. (6) Element F: Linking of new adaptive responses to new stimuli.

The volume concludes with a section entitled "How to make learning most economical and efficient," in which the author considers motivation, proper arrangement of stimulus-situations, effects of ventilation, illumination and fatigue, use of knowledge of goal and of score, and adjustment to variations in the curve of learning and modes of obtaining maximal transfer-effect.

The range of topics is wide, and many of the concepts developed should prove of value to students of education in replacing outmoded principles of a non-empirical pedagogy. Aside from a marked redundancy in style, the book is easy to read, and the arrangement is coherent. Particularly valuable is a final chapter in which an effort is made to bring together and to arrange in systematic order the major categories of the book.

As a textbook intended for students in elementary courses, then, the volume merits favorable consideration and should be of value to teachers of pedagogical courses. We read with amazement, however, the author's statement (v) that "the book should be of special interest to psychologists . . . interested in the scientific aspect of the psychology of learning since the attempt is made in it to bring together and interpret *all the facts about the learning process that have been gathered by the experimental studies of learning during the past thirty-five years*" (Italics mine). The briefest perusal of the volume will convince the casual reader that Book could have written this statement only in sheer irony and in a deliberate attempt at hyperbole. If the volume actually included reports of 'all the facts of learning' that thirty-five years of experimental work have produced, the field of learning would be in a parlous state indeed!

To become acutely aware of this, one need only tabulate the footnotes (as the present reviewer has done) to be convinced that the volume could have been written only as an elementary textbook. Out of slightly less than 200 footnotes documenting the author's references to the literature, there are some 70 references to the author's own publications. While one would not object to this in an elementary textbook, it seems slightly out of proportion in a comprehensive view of 'all the facts of learning,' since only one other authority is mentioned more than a bare half-dozen times and even this one exception is limited to 10 citations. Aside from Book's own contributions there are some 50 references for the decade 1922-1932, but close inspection shows that only about a third of these are taken from primary sources, the other two-thirds falling to textbooks and other secondary sources. The last five years are represented by half a dozen citations from primary sources; and there is nothing to show the student that scores of experimental articles have appeared in the journals each year during this period. In fact, as one turns the pages

of the volume, one is impressed by the lack of references to the more important of the modern experimental contributions, the chapter on the objective conditions of learning being a fair sample in this regard. The topic of forgetting is ignored altogether and the great mass of studies of verbal learning fares no better. The name of Ebbinghaus appears nowhere in the volume.

The author's use of apocryphal material, his free employment of the word 'law' to cover any principle he formulates, and his frequent strange liberties with logic may prove irritating to the critical reader. As an example of the first of this trio of irritants, we may quote briefly from the author's discussion of racial learning. "An animal species in its experience with its surroundings, like the individual members in their own more limited environment, is making progress in its ability to deal with its total environment. It is becoming more adaptable or, as we like to say, it is becoming more 'intelligent' as the ages roll by. *In a general way the individuals of each succeeding generation find themselves better able to cope with the difficulties that nature presents than were the individuals of the preceding generation*" (p. 9). (Italics mine.) After reading this, one shudders to think of the temporal proximity of the super-mouse and the super-oyster, to say nothing of the super-paramecium!

'Laws' are sprinkled so generously through the pages that space forbids detailed consideration of any of them; suffice it to say that any generalization, proved or unproved, appears to qualify as a 'law.' (Perhaps one should not be too severe with Book on this point, since certain of his contemporaries share his generous definition of this word.) We must content ourselves with a single illustration of his logical vagaries, leaving many others for the delectation of the critical reader. In discussing the effect of length of sleep upon work performed, Book writes; "No correlation was, therefore, found between the amount of sleep taken and the amount of work attempted and done. *In fact, those who slept the least seemed to do the most work*" (p. 393). (Italics mine.)

The reader may take exception to Book's continual use of *petitio principii* in resorting to the 'practical' whenever the going becomes difficult; and he will not be favorably impressed by the continual loose usage of such psychologically flavored terms as 'unconscious attention' (269), 'reversal of stimulation' (274), and 'efficiency' (429), the last being used where output alone is known. In fine, if the volume is intended for use as an elementary textbook in undergraduate courses, one may say only that it is superior in content and in organization to the vast majority of such publications. If, however, the author actually intends to offer it as a modern and comprehensive treatment of 'all the facts of learning,' he need expect little serious attention from critical researchers in that field.

Cornell University JOHN G. JENKINS

The Affective Consciousness. By H. H. BRITAN. New York, Macmillan Co., 1931. Pp. ix, 391.

The author of this book is Cobb Professor of Philosophy at Bates College, Lewiston, Maine. Its purpose is to present a systematic treatment of the affective consciousness, of which, the author states, there has been no such treatment since Ribot's *Psychology of the Emotions* was published in 1889. The text in mimeographed form was used as the basis of a course given to college students.

The word *affective* is used in its most comprehensive sense, as covering every-

thing conveyed by the words *affection* and *emotion* in psychological usage. The scope of the book may be indicated by the mention of a few of its sixteen chapters: The Motives of Men, Pain, Pleasantness and Unpleasantness, The Emotions, Fear, Imagination and Emotion, The Ethical Emotion, The Aesthetic Emotion, The Religious Emotion.

The author at the outset refuses to enter into the discussion of the merits or demerits of behaviorism, intellectualism, the instinct theory; and takes the position that his own theory, which is interactionistic, will justify itself by the truth it can produce, although it is not plain whether he means the truth that will appear in the remaining pages of his book or in future years. He escapes the necessity of taking sides in any instinct-habit controversy by saying, "we possess, *either by nature or nurture,* the tendencies to respond in definite ways." In fact, the book can be characterized by the word *irenic.* It constitutes four hundred pages of placid and benign discourse, in which controversies are merely hinted at. The students to whom the contents of this book were delivered, except the more original minds among them, must have passed from the room where the lectures were given, saying to themselves, "The professor is in his chair; all's well in the psychological world; although there is some rumor of rough weather there, it is only a passing squall." The most characteristic statement in the book, which epitomizes the entire production and might be used as a motto, may be found at the bottom of page 13: "It is not our purpose to take sides."

Laying down the principles that ideas alone are not always sufficient to produce action, that the motive values of ideas are derived from their affective tone, and that pleasure and pain act as selective factors, the writer proceeds to glide through page after page of psychological commonplace. While it is all expressed in a genial manner and in a style that is sprightly and sometimes pungent, the surface treatment invalidates the book as a text for use in advanced courses, which the writer intended it to be.

The author seems greatly exercised over the possible ill-effect upon the young, of the Freudian theory of the complex and of repression; and refers to it more than once. He shares the common mistake of supposing that Freud taught that repression is dangerous always and for everybody, despite the fact that Freud has protested that he never did say this, and calls for any statement of his that would justify the misconception. It is, of course, only in the case of neurotics that bad results may follow; but the neurotic is out of luck if he does repress and also if he does not. Normal people at Bates College and elsewhere will go on successfully repressing, as they have always done; and this only fear that ruffles the author's emotional calm, he may safely dismiss.

The last statement, however, may not be quite true, for the author does show some agitation over modern art-movements, such as Cubism and Futurism. He calls them psychological lies, because man, so he says, can never be apprehended in terms of cubes and angles. He combines with what seems to be a mimetic view of art another that seems to be conceptualistic, for the exact lineaments of great men or women portrayed in art mean little, unless they typify something more profound and universal than just one more member of the human species.

Consequently, the author would have to withhold the designation *aesthetic* from any experience that consisted in one's absorption in a painting that did not, to use

his words, lift the mind to Olympus and to fellowship with the gods; even if it did absorb the individual in some intriguing geometrical representation of the human form, so that he dropped his load of worldly care and enjoyed surcease for a time from the serious concerns of life, and all his mental processes were congruous with one another, and he did not have to do anything about it, except to enjoy the resulting repose. This latter view of the aesthetic experience, and of a reason for seeking it, would be to him unsatisfactory and even anathema, for the reason that art must make us noble.

Knox College R. T. WILTBANK

Die Psyche der weiblichen Jugend. By Else Croner. Pädagogisches Magazin. Lagensalza, Herman Beyer & Sons, 1930. Pp. 92.

In this volume the author, representing the intuitive-historical school of psychology founded by Eduard Spranger, supplements the latter's "Psychology of Youth" with valuable studies of female adolescence in several social strata. She compares puberty in the human species to revolution in the state and notes that the Crisis is more profound in woman than in man. After discussing the limits of the conception of adolescence, the author describes with great insight, the five principal psychological types among young women studying in higher institutions of learning. These types are characterized as: (1) Maternal, (2) Erotic, (3) Romantic, (4) Practical or Matter-of-Fact, and (5) Intellectual. The so-called Social type is regarded as a special form of the Maternal type, while the Artistic derives from the Romantic. In a section on the problem of individuality, the relation of the young girl to art, religion, love, and friendship is sympathetically interpreted in the light of a contrast between masculine and feminine attitudes toward life—woman's sense of fate versus man's sense of freedom. This leads up to a study of the sociological relationships of young girls within the home, school, profession, and society as a whole. Part I closes with a statement of the general laws governing mental and cultural development in girls. It appears that the sexes differ considerably in their response to the puberty situation. The period affects girls more profoundly. It does not result in intellectual retardation. A half tragic sense of devotion and willing sacrifice in the interest of the race appears and is peculiar to woman. Mental stagnation generally sets in at the beginning of the 20th year.

The second part of this work treats briefly the psychological development of adolescent girls of inferior social status—servants, factory workers, pupils of trade schools, etc. The same types are found here, but less clearly defined and the crises of the developmental years are less severe. An interesting classification of female domestics is presented: viz., (1) a submissive, trustworthy type perfectly adapted to their calling, (2) a reliable, but somewhat arrogant and militant type (Küchendragoner), (3) a flighty, vagrant type (Vagantin), and (4) the indifferent—along with sub and pseudo-types.

A third part offers a description of psychopathic types among young girls, which will be of considerable use to educators. The book closes with a characterization of the modern girl and the perils as well as advantages which surround her as a result of recent liberal educational methods.

Croner has a fascinating style and illustrates her principles by well-selected ex-

amples from the artistic productions of youth or of novelists who portray the life of youth. No work could create a more sympathetic atmosphere for the study of the modern girl. There are two bibliographies including in all 69 titles.

University of Cincinnati CHARLES M. DISERENS

Leistungsfähigkeit und Selbstbeanspruchung: Eine psychologische Betrachtung. By OTTO LIPMANN. Leipzig, J. A. Barth, 1932. Pp. 38.

This psychological essay is largely devoted to a painstaking clarification of a number of concepts important for those interested in the theoretical or practical aspects of the "science of work" (Arbeitswissenschaft). Lipmann has devoted years of effort to the elaboration of methods and concepts for use in a scientific approach to the problems of the human factor in economic life and has published only recently a comprehensive review of the entire field in his *"Lehrbuch der Arbeitswissenschaft."* The present study touches upon some of the topics elaborated in his book and brings out in bold relief essential connections and relations among such things as "fitness," "fatigue," "willingness-to-work" (Selbstbeanspruchung), "practice," and "sleep."

In a brief introduction, the problem is defined in James' terms, where James speaks in his "Energies of Men" of the importance of periodic fluctuations of man's fitness and 'energy,' and where he deplores the lack of adequate information on the subject. Lipmann demonstrates in his essay what progress has been made in our knowledge of these things since James' statement in 1906. The first two sections are devoted to the definition of terms and the third section to conditions of varying "willingness-to-work," such as the subnormal condition in depressive swings, or the importance of the habitual "willingness-to-work" as a distinctive trait of one's personality make-up. A brief consideration of phenomenological and dynamic differences of "fatigue," "inertia," and "laziness" follows. The interdependence, in normal conditions, of the individual's "readiness-to-work" and the number and quality of the tasks at hand is emphasized. The last section of the study deals with reactions of the organism, which produce a compensation for energy expenditures, such as "compensatory occupations," various ways of finding "recreational outlets," and "sleep."

The author is able to combine a precise usage of the technical terms with a nicety of style that makes for enjoyable reading.

Johns Hopkins University MAX F. HAUSMANN

Der Aufbau der Farbenwelt. By DAVID KATZ. Leipzig, J. A. Barth, 1930. Pp. xviii, 484.

This is a revision of the author's earlier book *Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung,* published in 1911, and, like the former volume, it is dedicated to his teacher, G. E. Müller. After an introductory chapter on the modes of appearance (*Erscheinungsweisen*) of colors and the phenomenology of illumination, Katz reports experiments with film colors, surface colors, and transparent colors. Though these first sections of the new book follow the general outline of the earlier volume, much of the material is new. A few of the later chapters are wholly new. In discussing the application of color phenomenology to the art of painting, the author points out that aerial

perspective is due not to the actual changes in the color tone of distant objects, but to the vagueness of their contours and the indistinctness of their details. In other words, the more distant the object, the less it appears as surface color and the more nearly it resembles a film color. Many old masters (Raphael, for example) failed to realize this and painted distant features of the landscape with the same sharpness of detail as objects near at hand. Some modern painters have shown great skill in indicating depth by the varying softness of contours, and have even taken account of the effect of accommodation.

Psychological problems involved in the technique of depicting illumination are also interestingly discussed. Subsequent chapters deal with color constancy and color contrast, measurements of the perception of illumination, and genetic psychology of color perceptions, including experiments with animals and children, and observations upon eidetic subjects. The final chapter discusses the color theories of Helmholtz, Von Kries, Hering, Bühler, Gelb, and others; and summarizes some of the author's own contributions to the field. Especially interesting is his discussion of the conditions under which chromatic illumination is perceived.

The book is generously documented with references to the research of other investigators. About 230 authors are cited.

West Springfield, Mass. MABEL F. MARTIN

Das Problem der Schülerbeurteilung. By A. BRUCKNER. Pädagogisches Magazin. Langensalza, Hermann Beyer & Sons, 1931. Pp. 90.

The author endeavors to find the relationships among three different measures of general intelligence taken on 200 students in the four upper classes of an "Oberrealschule" (approximate ages 14–19 yr.).

The students were asked to rank their fellow pupils according to general intelligence, and were warned in the instructions not to consider characteristics supposedly not included in general intelligence, such as ambition, industriousness, attentiveness, moral standards, and so forth. Students judged to have equal intelligence were to be given the same rank number. Teachers (about 6 or 8 for each student) were given essentially the same instructions. Along with the ratings, teachers and students were also asked to give some remarks explaining the reasons for their judgments. Two tests were selected as tests of intelligence: (1) a completion test from Tumlirz, in which 29 verbs were missing, and (2) an absurdity test. The criterion for scholastic attainment was the average mark at the end of the school year.

The following main correlations were obtained by the rank method (Spearman footrule): (a) between judgment of teachers and of students, $0.85 \pm .08$ to $0.97 \pm .03$; (b) between scholastic attainment and judgment of teachers, $0.85 \pm .07$ to $0.93 \pm .03$; (c) between scholastic attainment and judgment of fellow students, $0.64 \pm .13$ to $0.91 \pm .04$; (d) between test and judgment of students, $0.38 \pm .15$ to $0.69 \pm .10$; (e) between test and judgment of teachers, $0.27 \pm .17$ to $0.62 \pm .10$; (f) between test and scholastic attainment, $0.26 \pm .15$ to $0.60 \pm .10$.

The rather high correlations between ratings of teachers and students are consonant with most results from pooled ratings of 6 to 7 raters, but only when both

groups have been influenced by common social environment (reputation). Although the correlations between test intelligence and the two factors, ratings and grades, cannot be directly compared with similar correlations obtained in American investigations because of the differences both in methods of grading and kinds of tests employed, yet it seems that the correlations are relatively low, a fact which may be due to the failure to use a more comprehensive standard battery for measuring general intelligence. In regard to the method of rating, more accurate modern technique might have been employed.

It is to be regretted that the author seems not to have had access to the rather extensive and important American literature in his field. The bibliography of 26 titles contains not a single American reference. A study of the above type might have been made infinitely more useful to science if pains had been taken to secure an international outlook over the field.

Mooseheart Laboratory for Child Research MARTIN L. REYMERT

Sexualpathologie, für Mediziner, Juristen, und Psychologen. By V. KAFKA. Leipzig and Vienna, Franz Deuticke, 1932. Pp. 170.

This book is developed from the author's lectures to his medical students but these have been modified to suit the work also to psychologists and lawyers. The author is a serologist who has taught psychiatry and his leanings are largely in the direction of organic psychiatry rather than toward psychology. Nevertheless, there is much in this book to commend it to psychologists. It is a straightforward treatise, relatively modern in its attitudes toward sexual pathology, and the arrangement is such that the newer viewpoints come to the fore.

In many books of this sort quantitative changes in the physiological sex act are not deemed worthy of discussion, but in the present work there is a chapter dealing with such variations as the speed of reaching the climax or components, in so far as such changes are related to normal effects of age. Other physiological variations in function are emphasized in such conditions as early sexual development and nocturnal emission. Changes, such as premature puberty, which might be either normal or abnormal, are carefully differentiated according to the features which might indicate abnormality. There is also a discussion of the variations in the partnership in the sex act.

The body of the book is made up of conservative discussions of masturbation, the various forms of sexual inversion, and such pathology as exhibitionism, sadism, and fetishism. These disorders are analyzed largely from the historical point of view and also from the point of view of the person who might deal clinically with them. The psychological changes which accompany the sexual abnormality, and the significance of the abnormality itself are not touched upon to any great extent. The use of psychoanalytical treatment is not stressed. Therapy is dealt with in single sentences, such as "in such cases there is a need for deep therapy." The legal comments are brief and give us little psychological insight into the juristic attitudes involved.

For psychologists who are not familiar with the literature of this field and who wish a safe and sane description of sexual psychopathology the earlier chapters of this book will probably be found to be of most value. One-third of the book is occupied with a discussion of the sexual changes accompanying the psychoses,

the sexual picture presented by criminals, and the organic complications found in sexual pathology. The psychological material in this part is scarcely worthy of the attention of any other than a medical man.

Institute of Juvenile Research — LOWELL S. SELLING

Über das hierarchische Prinzip in der Natur und Seine Beziehungen zum Mechanismus-Vitalismus-Problem. By EWALD OLDEKOP. Reval, E. Wassermann, 1930. Pp. 64.

This is a theoretical analysis and discussion of the problem. The author distinguishes between "physical *Gestalten*" and "organic *Gestälten*" or unities. The former is strictly dependent on its outer "topography." If this topography is changed there is invariably a change in the physical *Gestalt,* which is mathematically calculable; but an organic *Gestalt* (while also not *independent*) is not so directly dependent on these external conditions, since these conditions may vary to certain extents without effecting corresponding changes in it. A physical *Gestalt* for similar behavior or functioning might well have had any one of an infinite number of other forms, but an organic form (*e.g.* an organism) "carries its end within itself" (in its germ-cell determiners) and the development of its several parts are somehow regulated by this 'end' or inner factor. That is, the system 'strives' to attain a certain predetermined structure independently of outer conditions.

Objectively considered, one might add, this difference would tend to become a question of the relative immediacy or remoteness of controls by external factors and cannot be regarded as proof of vitalistic (or in psychology of animistic) assumptions in the sense of original forces, whatever that may mean. Even all human purposes are outcomes of certain conditions. The author points out that in the organic unities, which evidently are determined so largely by the environment of early phylogenetic forms, the present environment acts more indirectly and allows room for interplay of trial and error impulses (preceding the overt acts), thus permitting what might be called intelligent adaptations. There is, unfortunately, no adequate consideration of much recent work—by C. M. Child, S. T. Bok, M. Minkowski, et al.—on physiological gradients and consequent regulation of nerve-fiber growth as affected by the requirements of adjustment to external conditions of light, food, etc.

There is no index. — J. P.

Zur Psychologie des musikalischen Gestaltens. By JULIUS BAHLE. Leipzig, Akademische Verlagsgesellschaft, 1930. Pp. 102.

Although the experimental controls do not meet the demands of the most rigorous researches of the laboratory, this study marks a forward step in the analysis of creative ability, especially in the field of music. We have first a scholarly and systematic presentation of previous approaches to the subject of musical creation and composition. The speculative method has contributed little to aesthetics and the meditations of artists and aestheticians give us little more than mythical interpretations of inspiration and embody the error of generalizing from one instance which does not possess the first ear-marks of adequate sampling. We are here reminded of the early diatribes of Wundt and others against uncontrolled self-observation. The empirical-historical techniques evaluated by Stern and the

interpretative historical point of view of Dilthey and his followers are merely mentioned in passing, to be relegated to the realm of the unreliable and incomplete. The pathopsychological treatments of Prinzhorn and Tramer show much more promise but are not specific enough in details. The theoretically psychological and the interrogatory methods point all the more to the advantages of a straightforward experimental technique with the aid of other methods by way of interpretation and suggestion.

The task was set to 13 *Os*, almost all of whom had already composed music more or less extensively, to express themselves musically in terms of eight problems: (1) their momentary mood, (2) some personal experience of an emotional sort, and the emotional complexes of (3) hilarity, (4) sadness, (5) suffering, (6) pride, (7) anger, and (8) hate. They were also to represent in musical characterizations, (1) a 'skin flint,' (2) a psychoesthenic, and (3) a conjurerer. They were to set down their mental processes.

In the progress of the experiment it became apparent that the *Aufgabe* to express certain emotions did not lead exclusively to this result, and the same principle applied to the problem of characterization. The observer fashioned at first a concrete situation from which to lead off some definite emotional experience. He then tried to translate this into musical form. This is designated as the method of emotional empathy or transformation. Another procedure was to arouse a complex of ideas or elaborative experiences which should indicate or represent the solution of the problem. A third method was to imagine a situation in which the observer was certain to feel the emotion called for. The writer goes on to elaborate on these three procedures and to quote extensively from the writings and biographies of noted composers to substantiate his findings. By means of diagrams the author shows how the composer draws upon musical forms which have been associated with similar feelings in past experience. Some of these are integrated, some are abstracted, and some are isolated reproductions of Gestalt factors. The monograph is rich in allusions to musical works and shows a comprehensive understanding of both psychological and musical problems.

University of Iowa CHRISTIAN A. RUCKMICK

Die Grundlagen der Charakterologie. By TH. ZIEHEN. Langensalza, Hermann Beyer & Sons (Beyer and Mann), 1930. Pp. 372.

Les Principes de la Caractérologie. By LUDWIG KLAGES. French translation by W. Reol. Paris, Félix Alcan, 1930. Pp. 263.

The book by Ziehen consists of a series of lectures on characterology delivered at the University of Jena. The lectures attempt to cover a wide variety of interrelated topics, and to summarize the present state of knowledge in a still very uncertain and controversial field. Developmental stages in character, sex differences, the relation between character and constitutional type, the character of the criminal and of men of genius, racial and occupational differences, the study of physiognomy and handwriting, and the experimental approach, are among the topics which receive detailed, though not always very critical, consideration. There are also a number of suggestions as to practical methods of controlling character, methods very strongly reminiscent of the attitudes of an

old-fashioned Puritan schoolmaster. The writer's personal contribution comes from his wide experience with pathological material, and his discussions are illuminated by case studies illustrating character trends in various clinical conditions. The quantitative studies which have occupied the attention of American students of character receive very inadequate consideration. The book is worth reading, however, as a convenient summary of the results of the more qualitative approach to problems of personality and character.

The first edition of the book by Klages, "Prinzipien der Charakterologie," appeared in 1910; the present French translation is of the 1928 edition. In the last twenty years Klages has achieved considerable prominence in the field of characterology, his excursions into graphology earning particularly wide attention. To the objectively-minded psychologist, however, this book will be something of a disappointment, as it is concerned mainly with metaphysical foundations, rarely tempered by anything resembling an empirical approach. Even the metaphysics has a somewhat antiquated Scholastic flavor; the discussion of the relative contributions of "Soul" and "Mind" to individual differences in personality seems more than a little out of date. Some of the other writings by the same author have made more substantial contributions to characterology as a potentially "positive" science.

Columbia University OTTO KLINEBERG

Psicologia dell'eta evolutiva (Infanzia e adolescenza). By ENZO BONAVENTURA. Lanciano, Giuseppe Carabba, 1931. Pp. 251.

This is an uncritical treatment of child and adolescent psychology, which does not assume a knowledge of elementary psychology. The author gives in the preliminary part of the book (about 45 pages) the basic facts and principles of elementary psychology. The rest of the book is divided into two parts: the first deals largely with the genesis of various mental functions, indicating roughly the periods of their appearance and the nature of their development. The second part outlines the nature and use of certain intelligence tests selected with especial reference to the general purpose of the book. A good deal of space is given to the description and clarification of certain psychological facts and principles. In a brief outline of the methods of psychology and psycho-pathology the introspective method is regarded as imposing obstacles to mental behavior in that one is requested to be both the observer and the observed. "If you are observing your fear you are not afraid; if afraid, you cannot observe." This is of course intended only as an example of the difficulty in a selected field; a critical evaluation of introspection is not attempted. For his present purpose the author prefers a 'dynamic-functional' approach. Development is best understood by objective, genetic studies. In Part 2, though older divisions in psychology are mainly used, the treatment is more modern in a number of respects than would be supposed from the chapter headings, but there is something of a dearth as to experimental facts. This may be excused on the basis of its elementary nature.

The author, in Part 2, after a general introduction on views and methods of evaluating intelligence, reproduces the various tests in the Binet scale (1911), the Kuhlmann for early infancy (from the 3-mo. to the 2-yr. levels inclusive), the Stanford-Binet revision by Terman, and he also refers to the testing work of Bober-

tag, Decroly, and Burt. The De Santis tests for abnormal children and interrogations (concerning children's interests, sentiments, orientations, associations, imaginations, desires, etc.) are also included, and the 47 questions by Ferrari and Francia (1912) are reproduced. From these questions, it is pointed out, one does not get a measure of intelligence; but they afford useful information and help to the teacher. In an analytical examination of the mental functions, various tests of these supposed functions are described. These include the Porteus Maze (learning) tests, certain absurdity tests (solution of problems), tests of memory, imagination, etc. Some of the absurdity tests would be new to American readers. Profile tests are also considered and discussed. All this material is presented, of course, without much critical evaluation. There are appendices on the examination of the senses and on the calculation of correlations.

Very few references are made to American authors and work, despite the many contributions of late years to childhood and adolescent psychology. Experimental work on learning is sadly neglected in the book. The 3-page bibliography at the end includes from America only Terman's *The Measurement of Intelligence,* and Thorndike's book by the same title.

J. P.

The Psychology of the Inventor: A Study of the Patentee. By JOSEPH ROSSMAN. New and Revised Edition. Washington, D.C., The Inventors Publishing Co., 1931. Pp. x, 252.

The title of this book is somewhat misleading; there is really only little psychology in the book. The first and second editions both appeared in the same year. In the latter edition the format has been changed and some corrections and additions made. The author contends that one of "the outstanding characteristics of our civilization is its complete dependence on invention." This view is justified by his extremely broad meaning of "invention," to include all novel productions along lines of human interest and not simply in the technical field. He rightly attributes the confusions of thought relating to *invention* to its many and vague meanings. Legally an invention has a rather restricted meaning: new principles not connected with mechanisms for application to practical purposes, "purely mental" things, mere ideas, systems of doing business, principles of nature, etc. are not patentable (p. 23). The list of things patentable is, however, extensive and cannot be reproduced here. Invention, legally, does not arise from reasoning or from any logical deduction, neither is "the exercise of ordinary mechanical skill" invention. To quote from the decision of a judge: "But if we create not only by the operation of mind, but as well actually, physically, new means by which is necessarily obtained a certain specific end, means which are novel to the creator as well as to the world, which had never existed before, at least in the combination or in conjunction in which the creator causes them to exist for the first time, he who does this must, I think, be regarded as an inventor" (24 f.). Indeed definition of invention is so difficult that the courts have had to adopt the opposite method, that of defining what is not invention. To determine whether some one's creation is an invention has therefore become a very complex and expensive process. If an invention is patented, there are so many pitfalls to success, including clever tricksters

who pretend to be interested in the patent and for some named sum and plausible reasons can help one get it placed or secured to the would-be purchaser, that most inventors find the business unprofitable. Consequently, with this increasing complexity, patents seem to be granted now mainly to engineers in the various industries and to engineer executives of such industries, and to consist more and more of improvements of a few older great and striking inventions. The government has evolved no ways of protecting the patentee: that burden must rest on himself.

Through questionnaires (unfortunately of a too general nature) to inventors, and from replies from 710 inventors with an average of 39.3 patents, the characteristics, methods, mental processes, occupations, motives, heredity, and training of inventors are investigated. Nothing that is probably not known results. There seems to be no special endowment, except a degree of general intellectual ability, revealed; this ability probably tending to run, as is well known, in families. It is clear that inventors know very little themselves about requisite abilities and even motives for invention. Unfortunately no detailed first-hand study of any inventive process is given. Such studies are certainly needed. Data of the desirable sort are only approximated in the report of one inventor—W. H. Smyth (63 ff.), working on the problem of soldering both ends of fruit cans simultaneously in the time previously required for one can; and these data certainly do not support the legal conception of an invention quoted above.

The author seems sound in his conclusion that there is no special sort of genius in the usual inventor, and that the procedure of invention is noting some defect in operation, some imperfection or some "need," which he sets about improving or supplying by a more or less trial and error method (even though he may have good technique within certain limits). Solutions often come about rather suddenly as if by chance observation of some principle in operation.

Due to the equipment of the author, and his extensive experience as Patent Examiner in the U.S. Patent Office, the book contains much information of particular interest to young inventors, and to research workers generally. The psychological theories of invention are stated very generally and are well known to psychologists. The interesting experimental work on invention begun by Josiah Royce, and extended to work on puzzles by E. H. Lindley (this JOURNAL, 8, 1897, 431 ff.) is not mentioned, even in the 10-page bibliography.

J. P.

The Measurement of Interest. By DOUGLAS FRYER. New York, Henry Holt & Co., 1931. Pp. xxxvi, 488.

The author has taken a very limited psychological field and attempted to survey everything that has been done in it. This effort is commendable and, the reviewer believes, has been eminently successful. Anyone wishing to acquire a background in this field of research will not have to seek any further.

The point of view of the book is essentially empirical. Fryer is chiefly concerned with the measurement of interests rather than with their theory. After differentiating between the subjective and objective aspects of interest, he treats of them separately. Then turning to more theoretical considerations the author points

out sources of error in interest inventories and in the objective measurements. He proposes his own theory along the lines of acceptance-rejection, on the basis of which he distinguishes between interest and motivation. He then outlines a program of research, during which he points out the possibilities of material obtained in autobiographies and clinical examinations.

The book closes with five appendices. The first sketches the historical trend with regard to interest in education and industry, and especially the latter; the second treats in similar fashion of subjective interests; the third and fourth give scoring keys for various interest inventories; and the fifth and last deals with the interests of several occupational groups.

The book is not of the type that will be read carefully from cover to cover, but is more like a handbook which the reader will sample according to his problems. The vocational counsellor will find the results of numerous studies in the vocational field and suggestive schedules for conducting interviews. The educational psychologist will find material on children's interest. The industrial psychologist or personnel worker will get the historical background of his field and will find many pertinent suggestions for measuring interests. The person devoted to research can take this book as a starting point, and by use of it avoid many mistakes that have been made by others.

Ohio State University HAROLD E. BURTT

Association Theory To-day: An Essay in Systematic Psychology. By EDWARD S. ROBINSON. New York, Century Co., 1932. Pp. x, 142.

The subject of this book, association theory, is a central one no matter what one's special interest in psychology. Here is a treatment of it which is eminently sane yet provocative of thought and of reconsideration of one's own position. The style is facile and bright, in general barely avoiding the colloquial; occasionally it rises to the epigrammatic. Only the lack of a certain distinction keeps it from being really first-rate prose.

Robinson does not give us a compendium of experimental facts such as in other fields we have had from Fröbes and from Murphy. Critical experiments, it is true, are cited at all times and experimental data are in the center of the author's eye—possibly too largely to the exclusion of non-laboratory experience. But it is, rather, a critical essay, chiefly dialectic. This statement, of course, will condemn the essay forthwith in the eyes of those who do not realize the polar position of speculation in scientific advance. Robinson's treatment, none the less, will help, both to give meaning to known facts and to disclose new ones.

There are two attitudes commonly taken toward an historic theory like associationism. Köhler, I think it is, has somewhere expressed one of these very clearly. No one, he says, will hold this theory as I have stated it, but many persons *should* hold it because it is the central position from which radiate the doctrines they have actually espoused; and unfortunately this central position is, as anyone can see, quite absurd. Effective as this sort of argument is, it degenerates easily into the playful kicking of the stuffings from a straw man.

Robinson takes the opposite tack. He, too, tries to find a central position, a sort of greatest common denominator for associationism. This common denominator, however, is so sensible a thing it is difficult to see how anyone can object to

it. "By association," he says, "we may mean simply the establishment of functional relations among psychological activities and states in the course of individual experience."

But of course each reliance upon a common denominator also has its weakness. Association certainly means that, but it also means more or we should all be associationists. Associationism is surely neither palpably absurd nor yet so sensible that none can deny it. Robinson himself proceeds at once to such concrete specifications as leave ample room for differences, and rightly so since it is doubtful whether a theory which cannot be denied has any meaning or value.

The reviewer admits to a marked feeling of impatience with associationism chiefly because of its relative sterility in practice. The doctrine is old enough by now to have borne more fruit. Perhaps this is because we have taken it seriously but not critically. Robinson proposes to take it critically and see what he can do with it. If the resulting systematic position is not strikingly original and therefore unlikely to arouse intense loyalties, it is a vigorous position which keeps close to the central tradition in psychology. Whether we supplant or supplement associationism, we shall do so more intelligently for this essay.

Ohio State University HORACE B. ENGLISH

Psychology for Advertisers. By D. B. LUCAS and C. E. BENSON. New York, Harper and Brothers, 1930. Pp. xv, 351.

The authors' aim, stated in the preface, is "to present the theoretical, though scientifically supported, psychological aspects of advertising. There are six parts, dealing with the "mental processes and natural tendencies of the customer," the "psychology of appeals," how to "construct the advertisement," the several "mediums for carrying appeals to customers," the "measurement of the effectiveness of advertising," etc. One chapter is given over to a discussion of "human differences," and the final chapter to "advertising and the thinking of the public."

The book is strongest where the authors' plan of presenting the experimentally supported psychological aspects of advertising is adhered to most closely. It is weak in the numerous instances of assertions made dogmatically with no hint to the "practical person" that corroborative evidence on these points is lacking. Magazine advertising is taken as the type exemplifying the modern trend in advertising because it "has made great gains in public approval and prestige. The advertisements have been improved in appearance, content, and in their wholesome frankness." Thus the stamp of "scientific" approval is placed on the advertising of such nostrums as those which claim to be panaceas for unpleasant breath, 'gingivitis,' athlete's foot, and dandruff.

The gratification of the authors in the "practical success" which advertising has enjoyed is evident throughout the book, and is expressed in the closing paragraph, which conveys the general tone: "Besides being responsible for the widespread reading of periodicals and other printed matter, it [advertising] has placed the radio in millions of American homes. For ten years a nation has thrilled at the magic of wireless and constant entertainment, all because industry is willing to supply that entertainment for the 'good will' we may reflect."

University of California at Los Angeles FRANK C. DAVIS

A Comparative Study of Those Who Report the Experience of the Divine Presence and Those Who Do Not. By ROBERT DANIEL SINCLAIR. University of Iowa Studies: Studies in Character. Iowa City, The University, 1930. Pp. 62.

The purpose of this study is to bring to light the hidden mechanisms that condition the mystical experience of those who report the "experience of the Divine Presence." For the solution of this problem a means had to be devised for the selection of two groups: the Positive, or those who had had such experience, and the control, or Negative group, those who had not had it. This took the form of a point-scale self-rating test in which the rater indicated the degree or quality of his experience by checking to the right or to the left of a colon at the center of two extremes, and also the degree of presence or the total absence of trait by the same checking device. A check on the reliability markings was made by using the chance-half method of correlation with the eight significant items for the whole distribution of 549 cases. The resulting correlation was 0.86 and, according to Brown's formula, the test as a whole had a statistical reliability of 0.92.

Fifty students representing the extreme mystics and fifty representing the extreme non-mystics were selected from the results of this point-scale self-rating test. These two groups were then each given a battery of tests to determine experimentally the differences between the groups in sensory characteristics, motor abilities, intellectual traits, and suggestibility.

The sensory tests consisted of measures of visual discrimination, weight discrimination, sensitivity to electric current, and auditory discrimination. In all these tests there were found no significant differences between the groups being studied.

The motor abilities were measured by a simple reaction-time test (Klopsteg chronoscope), a test for the rate of continuous movement, a test of muscle steadiness, of muscle coördination, of serial reaction-time, and of tracing mazes. Of these tests, those which involved little more than simple motor reactions showed no significant differences between the two groups; but when complicated reactions were involved, the Negative (control) group was markedly superior.

Other tests were the memory of the details of a picture, an Aussage test of memory, a slightly modified Woodworth-Wells direction test, a test of ethical discrimination, of abstract judgment, of spatial imagination, and one of intelligence. The average grade-points for the previous semester were also used for comparison. In every one of these, except the grade-points, the Negative group was far and reliably superior to the Positive group. In the study of suggestibility to electric current and to pictures, the members of the Positive group were much more open to leadings and promptings than the Negative group, the difference being reliable. Further study of the self-ratings by the point-scale indicated reliable sex differences and an interesting predominance of certain religious beliefs in the Positive group.

Peabody College MABEL F. NICHOLS

Educational Psychology. By WILLIAM CLARK TROW. New York, Houghton Mifflin Co., 1931. Pp. xv, 504.

Trow organized his *Educational Psychology* around a series of psychological nodes—"inherited nature, individual differences, and the learning process"—and

the principles developed are briefly applied to educational situations as the story of adjustment unfolds. Methods have not been described in detail, "for that," Trow claims, "is not the function of educational psychology" (p. 116).

The book includes a wealth of quantitative material in tabular and graphic form, so skilfully handled that the tables and figures do not encumber the exposition nor burden the reader with their weight. Many other features of the volume are no less commendable. It admirably reflects the skeptical attitude of present day science toward the concept of instinct; it presents in concise form the recent contributions of psychoanalysis to individual adjustment; shows clearly the present confusion regarding the nature of intelligence; describes the theory of rational learning of Gestalt Psychology; and it gives the results of the recent researches on the problems of growth.

The chief points of criticism are his implicit faith in the constancy of the IQ, his views of the influence of heredity which are still deeply dyed in Galtonism, and his basis for interpreting (as well as his method for computing) the coefficient of correlation.

Trow writes, however, with "the sweet insouciance of lettered ease." With the results of investigations at his elbow, he deftly and lucidly pictures the process of adjustment, and brightens his exposition with happy touches of his unfailing humor.

University of Chicago FREDERICK S. BREED

The Heart Rate. By ERNEST P. BOAS and ERNEST F. GOLDSCHMIDT. Springfield, Ill., Chas. C. Thomas, 1932. Pp. xi, 166; 68 fig.

This monograph emphasizes the necessity, in any adequate study of the heart rate, of making continuous records of the heart beats while the subject pursues his ordinary activities as well as when he rests and sleeps, and of making these records by some method which leaves the subject unaware that he is being observed. For these purposes Boas has constructed the cardiotachometer, described and pictured in the second chapter. (This instrument is now being manufactured by the General Radio Co., Cambridge, Mass.) The study reported is based on continuous observations, "in each case extending over many hours," of the heart rates of 356 adult persons, divided equally as to sex in the several groups. Records are presented of 103 normal persons, of 77 persons during anaesthesia and operations, and of various patients with cardiac insufficiency, valvular diseases, myocarditis, Graves' disease, neurogenic sinus tachycardia, auricular fibrillation, and complete heart block. The book begins with a short historical sketch of the development and techniques in the investigations of heart rates from the remarkable studies of Herophilus, the Alexandrian physician of the 3rd century, B.C., by means of an especially constructed water clock, to the present time.

Results by the present authors reveal variations of so enormous proportions in the normal subjects (range of ages 20 to 40 yr.; mean age of the 51 men, 28.1 yr., and of the 52 women, 24.6 yr.) that the question may well be raised as to what is really an abnormal pulse rate. The basal heart rate (taken in the "post-absorptive resting state, early in the morning, shortly after awakening, and usually before rising," p. 22) is compared with mean, maximum, and minimum rates during waking and also during sleeping hours. For men the means are 61.4 $\pm$ 8.22

(basal) and 77.8 ± 9.43 and 59.4 ± 7.50; for women 69.9 ± 9.44 (basal) and 83.9 ± 7.80 and 65.3 ± 7.12, respectively. Numerous other conditions of which the pulse rate is a function are considered, and results are frequently compared with earlier measurements. Numerous graphs show changes in heart rate in normal and diseased individuals through periods of many successive hours. The book is valuable to psychologists investigating interrelations of various organic changes. Detailed data on individual cases are given in the appendix. There is a bibliography of 180 titles.

J. P.

Naturgeschichte der Seele und Ihres Bewusstwerdens. By EUGEN BLEULER. Revised ed. Berlin, Springer, 1932. Pp. xxvii, 268.

The subtitle, in English translation, is "mnemistic biopsychology," and perhaps conveys a clearer meaning than the main title. The long preface asserts that the book is not essentially different from its first edition. Psychology is introduced as a branch of biology correlated with "the remainder of" physiology and pathology. The author claims to have been the first to reject the kind of "localization" proclaimed by Meynert and Munk, and to substitute the modern neurological view of a purely acquired localization of function in any individual's nervous system. For what kind of reader the book is written, is difficult to understand. Certainly not for the immature college student. The style is conversational and the topics discussed are such as might become the subject of a discussion between two psychiatrists meeting accidentally in a club. This is not said in order to belittle the book, but to make clear what the reader may look for when he opens it. For example: "The 'psyche' is a brain function; and a borderline between the 'psyche' and brain functioning cannot be drawn." Those who are interested in "insight" will find a discussion of "the significance of consciousness and insight" to their liking. Other important topics are memory, perception, abstraction, illusion, intelligence, logical necessity, space, time, psychical energy. The book is dedicated to the memory of Goethe, deceased just a hundred years ago. This also helps to characterize the book for the prospective reader.

University of Missouri MAX F. MEYER

Psychologie. By ALOIS HÖFLER. 2nd ed. Edited by Aloys Wenzl. Vol. I. Vienna and Leipzig: Hölder-Pichler-Tempsky A. G., 1930. Pp. xv, 642.

The first edition of Höfler's *Psychologie* appeared in one volume in 1897. The manuscript for this new edition in two volumes was nearly completed when Höfler died in 1922. Wenzl has brought the work into its final form and added notes of his own. In the late nineties the work was widely known, not only because psychological treatises were then few, but also because it represented an authoritative account of the Austrian act-psychology which was then exerting great influence. The general systematic position of the new edition is the same, although there are shifts of emphasis. At the time of the first edition, the aim was to combat the dogmatic generalizations of associationism. Now, with associationism no longer a serious problem, the author is faced with the task of resisting the increasing wave of *a priori* and intuitional thinking which has swept over German psychology.

For Höfler the four basic classes of mental phenomena are ideations, judgments,

feelings, and desires. The first volume of the new edition deals only with ideations. Few psychological works are as thoroughly documented as Höfler's and few psychological writers have equalled his ability in imparting information at the same time simply and critically. Even the footnotes are readable. In spite of its literary and scholarly excellence, the work seems now to savor of the past. Its historical value, however, makes it a most useful work for reference.

University of Rochester C. W. Fox

Behavior Aspects of Child Conduct. By ESTHER LORING RICHARDS. New York, Macmillan Co., 1932. Pp. xv, 299.

This book contains a series of lectures delivered by the author in 1929 to the Baltimore branches of the Child Study Association of America. Although the book offers nothing new, it is valuable for its emphasis on the need for *interpreting* behavior rather than *judging* conduct. For the interpretation of behavior, no single viewpoint is adequate: the contributions of the psychologist, the physician, the social worker, the teacher, the clergyman, and the parents must all be given due weight. Psychoanalysis is dismissed as of comparatively slight value: in the analysis of psychological strains, it is usually not necessary "to resort to tricks of string galvanometer, or hypnosis, or dream analysis. The causes of strain are, nine times out of ten, to be found in a little careful scrutiny of situations at home, in school, and on the playground, and of the personalities concerned in these situations" (p. 141).

The book gives the customary emphasis to early childhood habits, the danger of discussing behavior problems in the presence of the child, etc. As in most lectures and popular presentations, the style is rambling and rather repetitious. There has been some of the usual pedagogical over-simplification of the difficulties in treating real behavior problems. The book contains an eleven-page index, but no bibliography.

University of California HERBERT S. CONRAD

Whole and Part Methods in Trial and Error Learning. By ELLA MAY HANAWALT. Comparative Psychology Monographs. Baltimore, Johns Hopkins Press, 1931. Pp. 65.

This monograph contains a report of a carefully planned and executed experiment conducted in the Michigan Psychological Laboratory on 15 trained male albino rats. Five pairs of maze patterns were used. Each maze was built so that it could be used as a single whole maze or as four separate smaller mazes, or parts, each leading into the food box. Five types of part procedure in learning were compared with the 'whole' procedure. These were designated as the pure part, the progressive part, the direct repetitive part, the reversed repetitive part, and a modification of the repetitive part. One pair of mazes and two groups of rats were used in making each comparison. The rats in one group learned one maze by the whole and afterwards the other maze by the part procedure, and the other group reversed this order, learning by the part procedure first. The criteria for comparing the results included the number of trials, the amount of time, the number of errors, and the total distance run. The last criterion was considered by the experimenter to be the most significant.

The results, expressed in both percentages and absolute measures, indicate that

the whole method of learning the maze is superior to either of the part methods. The order of efficiency in this experiment for the various methods was found to be whole, direct repetitive part, pure part, progressive part, reversed repetitive part, the modification of the reversed repetitive part. Results of this and of related studies by other investigators are not in entire agreement and seem to indicate that the optimum procedure may not be the same for all kinds of learning.

Peabody College EDGAR M. COOK

Mr. Eliot Among the Nightingales. By LOUIS GRUDIN. Paris, Lawrence Drake, Covici Friede, Inc., New York, American Agents, 1932. Pp. 71.

This essay by Grudin is a critical analysis of T. S. Eliot's recent essay *Poetry and Propaganda.* Eliot is recognized as an able representative of a group of writers who oppose modern mechanistic tendencies in science and philosophy with "a philosophy," to use Grudin's own words, "consisting of aesthetic intuitions" (p. 2). Grudin charges that Eliot, by the evasiveness of his problem, is involved "in practically every quandary of philosophy." He is forced by his method to seek illustrations from the intuitions of poets, from religious feelings, and from animistic and other vague notions to justify his views. "His procedure has been that of an excursion for words and notions to support his claims, wherever he could find them"; and he "has had to trust to meanings he could read into already available odds and ends belonging to various fields and gathered into a makeshift of critical doctrine. This is unfortunately the usual procedure followed by our literary critics" (p. 35). M. Grudin is himself the author of *A Primer of Aesthetics,* and has made this analysis of Mr. Eliot's essay largely to illustrate the dialectical method which he has been interested in developing to parallel the advances made in modern scientific method.

Would not some of the problems raised in these essays make a fertile field for psychological experiment employing quantitative methods of evaluating responses to certain objective situations?

J. P.

Criminal Statistics in Iowa. By CHARLES N. BURROWS. University of Iowa Studies: Studies in Social Science. Iowa City, The University, 1930. Pp. 112.

This study covers a period of 75 yr., from 1849-1927, inclusive. There is a graph and a statistical table for each county and four summary graphs. One graph shows the average criminal rates for the entire state of Iowa; the other three present the average criminal rates for the twenty-one most urban counties, the twenty-one middle counties, and the twenty-one most rural counties. The rate of crime, both of convictions and of sentences, is, in general, highest in the urban and lowest in the rural counties. The sources of material are the reports of the Secretary of State of Iowa, in relation to the criminal returns; and the reports of the Board of Parole of Iowa, with abstracts of the criminal statistics.

It cannot be too strongly emphasized that valuable as comparative tables may be for certain minor purposes, the percentages of the population convicted for crime and of persons sent to reform schools and penal institutions have no significance at all in themselves. Methods of dealing with and preventing crime have changed

markedly in the period covered and no one knows whether an increase in the percentages really reflects a worse condition than a decrease. This is not overlooked by the author.

Peabody College KENNETH B. HAIT

A Challenge to Neurasthenia. By DORIS MARY ARMITAGE. Second Edition, Revised and Enlarged. London, Williams & Norgate, 1931. Pp. 64.

This little volume gives a popular summary of the treatment of neurasthenia used by Dr. L. S. Barnes who died suddenly in 1927. It is written from the patient's point of view and is practical, helpful, and interesting. The urgent need of sympathetic understanding of the neurasthenic is stressed. The etiology of the nervous disorders considered is not dealt with other than by saying, "Dr. Barnes found the origin of all neurasthenic troubles in fear."

University of Wisconsin KAI JENSEN

Interest and Ability in Reading. By ARTHUR I. GATES. New York, Macmillan Co., 1930. Pp. xii, 264.

This is a report of further progress made by Gates and his associates in studies of materials and methods of teaching reading. The book is written in two parts. Part I presents and interprets the results from experimental studies of various factors which influence interest and ability. Part II formulates the principles which should underly the teaching of reading and gives illustrations of the methods which have been found to be most efficacious in producing the desired results.

Perhaps in no other direction have the fruits of the application of psychological analysis and experimental techniques been more gratifying than in that of reading. The rule-of-thumb and intuitional methods of an earlier day are being supplanted in our more progressive school systems by methods like those described in this book.

University of California at Los Angeles FRANK C. DAVIS

Education of the Backward Child. By DAVID KENNEDY-FRASER. New York, D. Appleton & Co., 1932. Pp. viii, 236.

This is an interesting and practical book for the teacher concerned with the education of the child having an I.Q. between 50 and 80. It is the outcome of many years of experience in the training of teachers of mental defectives in Scotland. The author shows that every child should be given tasks commensurate with his inborn capacity so that he will succeed and receive the "feeling of contentment arising from suitable work well executed." Even backward children have the necessary mental energy to acquire enough of the rudiments of knowledge to become socially useful if this energy is 'canalized.' This is best accomplished by presenting work in such a manner that it will be carried out in the spirit of play. The project method is found to be very useful here. Many concrete examples are given of methods of teaching oral language, writing, spelling, reading, composition, and numbers. Many of the suggested methods would apply just as well to teaching the normal child who is beginning the study of these subjects.

Wesleyan College, Macon, Ga. RALEIGH M. DRAKE

Psychologie der Werbung. By KARL MARBE. Poeschel. Stuttgart, 1931. Pp. 132.

The first half of the book is a general psychology written in very popular style. The other half is a technical discussion of particular problems in advertizing which the author and students working under his directions happened to take up. The psychologist looking for suggestions concerning the effectiveness of show-windows and concerning methods of book selling, may find himself rewarded by a careful reading of this second half, which has the appearance of being written for a period of business depression although it was published six years ago.

University of Missouri MAX F. MEYER

Die Psychologie des Grundschulkindes in ihrer Beziehung zur kindlichen Gesamtentwicklung. By OSWALD KROH. Eighth edition. Langensalza, Hermann Beyer & Söhne, 1930. Pp. 352.

In this new edition of Kroh's well-known book on child psychology, only a few minor changes from earlier editions have been made. As formerly, there is an introductory section in which the history of the growth of interest in child development is briefly surveyed. This is followed by a longitudinal description of the general course of development during childhood and early adolescence. The third and longest section is devoted to a topical account of such aspects of the child's mental life as the awareness of self, intellectual development, observation and perception, memory (with particular reference to eidetic imagery), the development of social and religious life, emotion and its expression, etc. Throughout, the significance of the scientific study of child development as a basis for practical education and training is emphasized. There is a short list of selected references, chiefly German.

University of Minnesota FLORENCE L. GOODENOUGH

The Indispensable Soul. By WILLIAM H. CRAWSHAW. New York, Macmillan Co., 1931. Pp. v, 315.

This book contains a goodly amount of material, apparently collected by the author over a considerable period of years and arranged by him in support of the thesis that the soul is indispensable in any adequate account of the world as a whole (including not only mechanisms and the animal mind, but also the moral, aesthetic, intellectual, and spiritual life of man). Though many of us may be sympathetic with this point of view, it must be noted that the author seems to take delight in neglecting to define his terms. There is also a tendency to vague, but emotionally attractive statements, and such scientific authority as is quoted is used in so general a manner that, even when it is authentic, nothing in particular is supported by it. Some men may prefer a shot gun to a rifle, but it is much less efficient for big game. Crawshaw is Dean Emeritus and Professor Emeritus of General Literature at Colgate University, and, as Socrates remarked of Polus, in the "Gorgias," has apparently "attended more to the art which is called rhetoric than to dialectic." The merit of the book is that it is a sincere and rather attractive confession of faith.

Hobart College FOSTER P. BOSWELL

Psychologie der Gegenwart. By H. HENNING. 2nd ed. Leipzig, Alfred Kröner. 1932. Pp. xi, 211.

This edition differs in no essential respect from the first published in 1925. Henning gives us, as before, a brief summary of the development of the newer psychology, followed by a survey of the principal problems and points of view in more than a score of the recognized fields in which psychologists have been active. In a few cases new paragraphs have been added to bring the chapters up to date, and a chapter on characterology has been included, but the principal improvement lies in the addition of new bibliographical material. In several cases, however, citation is not of the most recent edition. In both text and bibliography primary emphasis is laid upon German contributions, and an appendix on practical problems connected with the study of psychology can be of value only to German students.

Cornell University R. B. MACLEOD

The Rôle of Kinesthesis in Maze Learning. By D. A. MCFARLANE. Berkeley, University of California Press, 1930. Pp. 277-305.

This experiment was made upon the assumption that the kinesthetic stimuli in swimming a maze are materially different from those received in running a maze. This being true, if kinesthesis is an essential factor in maze learning, as it is generally considered to be, there should be no transfer from one kinesthetic situation to the other. The usual control was employed to keep all other sensory features constant while kinesthesis only was changed.

The experimenter found that so marked a change in the kinesthetic situation as shifting from running the maze to swimming the maze and from swimming to running had little effect on the ability of the white rat to traverse the maze.

It is urged that in studying kinesthesis in maze learning, a distinction should be made between the ability of the rat to know where its food is and those elements in the situation that determine its manner of reaching the food. The efficacy of kinesthesis is doubted in the latter and denied in the former.

Recognizing the value of the method used in the experiment, we doubt the validity of the assumption upon which it is based. There are probably a number of muscles, tendons, and joints used in common by the animals in running and swimming the maze; the rat responds as a total organism, and not in disintegrated form as here implied. It is questionable if the rat 'knows' where the food box is, apart from the kinesthetic elements which enable it to determine the proper manner of reaching the food.

Southern Methodist University JOSEPH U. YARBOROUGH

Information and Certainty in Political Opinions: A Study of University Students during a Campaign. By HAROLD S. CARLSON. University of Iowa Studies. Iowa City, The University, 1931. Pp. 48.

Among the recent investigations into determinants of opinion, Carlson's contribution will rank high. It indicates that the certainty with which opinions are held by 357 students is not an index of the information possessed relative to the political questions at issue. Analysis of data obtained through rating sheets, information tests,

and a questionary, yields contrasts in certainty and information among the following groups: voters and non-voters; students who formed preferences for a candidate early or late in a political campaign; one party and mixed party groups; sex; groups differentiated by Iowa Placement Examinations; and groups separated by years of academic advancement.

One of the merits of the study is adequate statistical treatment of the data. Three appendices supplement the main report; these concern, respectively, related experimental literature, the instruments used in obtaining data, and statistical considerations regarding the measures obtained.

University of Minnesota CHARLES BIRD

Attitudes of Children Toward Law. By EARL G. LOCKHARDT. University of Iowa Studies: Studies in Character. Iowa City, The University, 1930. Pp. 61.

This study is an attempt to discover the attitudes of 'modern youth' toward certain social standards of today by applying an objective test to a representative group of 3500 Iowa school children in grades 4–9. The test consisted of 51 questions based on the ethics of obeying or breaking 20 laws—more or less familiar to children—when the situations involving the laws supply a motive for disobedience. Only those questions were retained (from a longer list of 60) which received identical answers when presented to the 50 most outstanding lawyers in Iowa. The children's answers were scored "Right" if they agreed with those of the lawyers, and "Wrong" if they differed. A reliability coefficient of 0.837 was obtained by correlating odd and even halves of 100 tests of the heterogeneous group selected at random.

To check possible influences on children's attitudes toward law, Form C of Sims Score Card for Socio-Economic Status and Form A of the Otis Intelligence Test were given. Sex was also noted as a possible factor. These tests indicate (1) that children of both sexes and in all social and civic conditions have approximately the same attitudes toward law as adults; and (2) that intelligence is not a factor in determining attitudes provided the mental age is sufficiently high to make possible the comprehension of the law involved.

Board of Education, Cleveland FRANCES E. LOWELL

The Mind: A Key to the Interpretation of Psychical Phenomena. By GEORGE E. REHBERGER. Phoenix, Md., Lord Baltimore Press, 1931. Pp. vi, 214.

This work is a deluded attempt by a physician to explain all of normal and abnormal psychology on the basis of the author's analysis of a few of his own dreams. The importance of his "discovery of the mechanism of dreams" and the blind conceit with which this discovery is used as a key to psychology reminds one strongly of little Jack Horner. The author apparently knows nothing of Freud's contributions to the theories of dreams, and his acquaintance with psychological literature in general is about as broad as that of the beginning student in psychology, although psychological terms are used freely—and loosely. The book contributes nothing from a medical standpoint. Altogether it is a weird combination of misstatements and wild speculation with a minor degree of plausibility. Students in advanced classes might find criticism of the book a valuable exercise in clarifying their own thinking on some phases of psychology.

George Peabody College J. E. EARL

Jewish Experiences in America: Suggestions for Study of Jewish Relations with Non-Jews. Edited by BRUNO LASKER. Various contributors. New York, The Inquiry, 1930. Pp. xiv, 309.

This is a book of illustrative discussions, facts, opinions, attitudes, suggestions for leaders, outlines, etc., prepared by an "undenominational organization" for discussion groups among Jewish people with the purpose, apparently, of developing better understanding, better attitudes, and better general human relations among different religious and national groups in America. This book is one of a series put out by *The Inquiry* with the purpose of promoting "deeper insight into the causes of social friction and more effective methods of dealing with it." The series is necessarily of a popular nature since prejudice is usually associated with ignorance.

The book represents the results of considerable rather thoughtful experience—if not scientifically controlled experiments—and it contains valuable materials and suggestions on group psychology. There is a 22-page reading list (mostly of Jewish literature), but, unfortunately, no index.

J. P.

The Abolition of Poverty. By HARRISON E. FRYBERGER. New York, Advance Publishing Co., 1931. Pp. v, 152.

This book is written from the economic point of view and psychological aspects of the problem are neglected. Poverty and plenty are not clearly defined, and are used only as relative terms. Ten definite measures for the abolition of poverty are listed, and three others advocated. None of these measures is original, and some are hardly compatible; for example, the contradiction inherent in the advocacy of competition among independent small stores and the discouragement of competition among producers by means of a protective tariff leaves the reader wondering. The neglect of the economic effect of foreign loans likewise leaves much to be desired.

The book seems to be the result of private reflections of a rather well informed moralist. It has the merit and the limitations of such a work.

Teachers College, Columbia University HOMER E. COOPER

The Dissatisfied Worker. By V. E. FISHER and JOSEPH V. HANNA. New York, The Macmillan Co., 1931. Pp. ix, 260.

This book deals with the emotional maladjustments which handicap the individual in making vocational adjustments, however well equipped for his work he may be by virtue of his native intelligence and technical training. The emotional problems particularly stressed by the authors are: feelings of inferiority with their compensatory mechanisms, fears, and difficulties of the sexual life. It is pointed out that a large proportion of vocational maladjustments are only a part of larger emotional problems which affect the person in all his relationships as well as in the work situation. The emotional difficulties of the individual are expressed, in his work relationships, in jealousy of other workers, lack of cheerful coöperation, self-pity, complaints of unfair treatment, labor agitation, bullying of subordinates, fault-finding, frequent changes of jobs and other manifestations of discontent. In the more serious cases of emotional maladjustment, there are also more serious vocational difficulties in the form of industrial accidents or neuroses.

The nature and causes of emotional maladjustments and their effect upon voca-

tional adjustment are briefly and simply explained, and there are plenty of case study illustrations to supplement the general discussion.

Philadelphia Child Guidance Clinic — PHYLLIS BLANCHARD

The Sex Technique in Marriage. By ISABEL EMSLIE HUTTON. New York, Emerson Books, Inc., 1932. Pp. xv, 156.

In nontechnical language, and in a simple and direct style, Dr. Hutton places before her readers the psychological as well as the anatomical and physiological facts of the normal sex life. The book is written in good taste. It contains nothing of an erotic nature—except perhaps its misleading title—which may offend the sense of delicacy of its readers. The information imparted is in accordance with enlightened opinion. The book may safely be recommended to all who wish accurate information regarding the sexual life—to the adolescent as well as to those entering the married state.

K. M. D.

The Nervous System: An Elementary Handbook of the Anatomy and Physiology of the Nervous System. By JAMES DUNLOP LICKLEY. London, Longmans, Green & Co., 1931. Pp. xii, 144.

This is a new edition of the book that appeared in 1912. It contains a brief summary of the anatomy of the nervous system which differs very little from that given in the earlier edition. The physiology is orthodox and very elemental. The book as a whole may be characterized as a collection of the tenets of neurological faith, which have little value to the psychologist, and it is doubtful whether they should be taught his students.

Columbia University — W. N. CHAPPELL

Der Strafprozess gegen Philipp Halsmann: Aktenmässige Darstellung und Kriminalpsychologische Würdigung. By KARL MARBE. Leipzig, C. L. Hirschfeld, 1932. Pp. 163.

A review of the evidence and of the court procedure in the case of a pathological patricide from the standpoint of criminal psychology.

Trinity College — ROBT. B. W. HUTT

The Development of the Gonads in the Gold Fish, Carassius auratus (L.). By FRANK A. STROMSTEN. University of Iowa Studies in Natural History, Vol. XIII, No. 7. Iowa City, The University, 1931. Pp. 45.

This is purely a biological contribution such as would interest embryologists and histologists. Nothing bearing on sex behavior is given.

Stanford University — CALVIN P. STONE

Introduction to the Use of Standard Tests. By SIDNEY L. PRESSEY and LUELLA COLE PRESSEY. Revised Edition. Yonkers-on-Hudson, World Book Co., 1931, Pp. vi, 266.

The parts, chapters, and topics are identical with those of the first edition of this well-known book, with two minor exceptions in the general contents and one in the appendices. The topic of mathematics is given the space formerly allotted to

algebra and geometry, a page and a half of new material on general science is added, and Appendix E, consisting of less than four pages on the "new-type" examination, is inserted. Occasionally, in topic discussions, citations of typical tests have been changed; *e.g.* "The Gray Oral Reading Check Tests are the best example in this field," takes the place of "The Gray Oral Reading Scale is perhaps the best example of an oral reading test." The expressed purpose, philosophy, interpretation, and exhortations regarding testing are virtually the same as outlined in the 1922 edition.

Milligan College JESS H. EDDS

Soziale Seelenhaltungen. By W. STEINBERG. Munich, Ernst Reinhardt, 1932, Pp. 151.

Three basic forms of subjective life are being distinguished: Psyche, soul, and spirit (Psyche, Seele, Geist). Psyche designates the totality of subjective modes of existence exhibited by man as a natural (*naturhaftes*) living being. Soul and Spirit are put in contrast to Psyche, as peculiarly human phases of subjective existence, soul implying more the emotional, spirit the rational mode of life. Much space is devoted to a discussion of the various manifestations of these basic forms in social attitudes, such as striving for social position, love, hate, etc. The discussion is almost purely philosophical, with little or no reference to the body of knowledge of experimental psychology and sociology.

The Mooseheart Laboratory for Child Research MARTIN L. REYMERT

A Sociological Analysis of Certain Types of Patriotism. By EARLE L. HUNTER. New York, Paul Meisel Co., 1932. Pp. 263.

This volume is a doctor's thesis. The author attempts what he terms an inductive approach to the meaning of patriotism through an analysis of excerpts from various news items, editorials, and letters to editors gathered from the press of the country. He differentiates six types of patriotism which he names respectively, loyalty to state supremacy, institutional loyalty, national egocentrism, eclectic institutional loyalty, critical-mindedness toward the state, and national self-sufficiency.

The book is mainly sociological, interspersed with excursions into the field of political science. Although the author regards his work as an inductive treatment of the data he has collected there is in his inferences a tendency toward eisegesis, and it is difficult to escape the conclusion that his real purpose is to set forth certain preconceived notions, social and political, and that he uses his data as useful tools to corroborate them.

Aside from a few references to the psychological factors of patriotism the book is of little interest to the psychologist.

West Virginia University JOHN E. WINTER

The Cardiac Output of Man in Health and Disease. By ARTHUR GROLLMAN. Springfield, Ill., Charles C Thomas, 1932. Pp. xiv, 325, 15 figures in text.

This book is written chiefly for the medical profession. An excellent review, with criticisms, is given of the various methods used in the determination of the cardiac output. One chapter deals with the physiological variations affecting output, such factors as posture, sleep, menstrual cycle, psychic disturbances and temperature

being mentioned. Stimuli resulting in such psychic disturbances as resentment, regret, and anger were found to increase the output. Applications of cardiac output measurements in pharmocology, physical therapy and clinical medicine are fully discussed. A bibliography of 483 titles is appended.

University of California C. W. BROWN

Child Psychology. By BUFORD J. JOHNSON. Springfield, Ill., Charles C. Thomas, 1932. Pp. xiv, 439.

In this volume, Johnson has brought together in extremely concise and well-documented form a large body of experimental data on the development and behavior of the young child. The approach is topical rather than integrative. The subject matter is drawn from the laboratory rather than from the philosopher's arm chair. The method of presentation presupposes an extensive acquaintance with the methodology and the vocabulary of child development research; a fact that probably puts the book beyond the grasp of any except the advanced student.

The main body of the book is divided into eleven chapters as follows: periods of growth, learning, infant responses, locomotion, manipulation, speech, attention and perception, thought, emotion, social behavior and personality, individual differences. There is a bibliography of 206 titles and an index.

University of Minnesota FLORENCE L. GOODENOUGH

The Wholesale Personality. By W. H. BURNHAM. New York and London, D. Appleton & Co., 1932. Pp. xv, 713.

The volume is a contribution in the field of mental hygiene, much on the order of *The Normal Mind* by the same author. The work surveys the whole field of personality mainly by bringing together the results of some of the most scientific studies available on the topic. The author emphasizes the biological basis of personality and argues against dualism. Personality, for Burnham, is "largely a social evaluation" of the organism's behavior in the light of consistency and wholeness. The importance of early training in normal development is emphasized with illustrations, and some space is devoted to the known and probable influences of glandular secretions on personality.

For the lay reader, the author has implied too much, whereas the trained mental hygienist will no doubt view the contents as sound information commonly known. The anxious lay reader, looking for suggestions, will probably finish the volume having discarded many of his former fantastic notions of personality. The book will assist any reader to a better understanding of his fellow man as well as of himself.

Milligan College JESS H. EDDS

The Problem of Genius. By WILHELM LANGE-EICHBAUM. Translated by Eden and Cedar Paul. New York, Macmillan Co., 1932. Pp. xix, 187.

This is a translation of *Das Genie-Problem, eine Einführung* (1930-1931) which has been reviewed in these columns (this JOURNAL, 44, 1932, 483). This book is a restatement of the theoretical conclusions arrived at in Lange-Eichbaum's larger book, *Genie, Irrsinn, und Ruhm,* which contains the biographies of over 800 persons "who have been regarded as geniuses." J. P.

By Word of Mouth: Informal Essays on Language and Psychology from a Teacher's Point of View. By TATIANA W. BOLDYREFF. Boston, R. D. Badger, 1931. Pp. 144.

The author characterizes himself as a "Russian exile and a nomad," having an interest in mother tongues and "other tongues" because of experience as a student of languages and naturalization in America, service as a translator, and, finally, observations as a teacher of languages. He is disappointed at the small degree of attention that psychological investigation has devoted to language and at the present results, but attempts only vaguely to indicate specific well-thought-out problems for investigation. In view of what little has been done scientifically this vagueness is not surprising. Suggestions are made with respect to bilingualism. The author does not believe the learning of an artificial language like *esperanto* would be successful, and he favors making one of the modern languages common to all nationalities, while each language group retains its one native tongue. An artificial language interferes with freedom of expression; it is an impoverished combination of elements from several natural languages, and hence stifles thought, and imposes intellectual and social limitations. Moreover, such an artificial language "would also greatly injure, perhaps, eventually totally dispense with the study of foreign languages." As to the question of whether two languages learned simultaneously would be mutually inhibitive, the book reveals an ignorance of some relevant psychological experiments.

There are several other indications of a lack of contact by the author with psychological literature and methods. We read that in learning a natural language "there is also a certain guiding principle developing into something like inherited linguistic instinct or intuition which puts one through" (p. 21). It is interesting to note that the author, an experienced linguist and teacher of language, should fail to see more accurately what this 'guiding principle' really is. The magic of the term 'instinct' as a general explanatory means seems to appeal to him. "Existence of slang, richness in certain languages of idioms, fondness of figures of speech, and so forth, what are these traits but indications of the *bilingual instinct in embryo?* And yet man is a social being and it is natural for him to adapt himself to his fellow creatures of another nation including their peculiarities of speech. That is what the bilingual instinct *was meant for*" (p. 26. Italincs mine). These are examples of the author's naïveté as to scientific attitude; and still one must admit that there are good ideas, and that psychology can profitably investigate problems relating to the development of language in a broader and more scientific sense than heretofore. The child clinics recently developed in America are already making young subjects more available for experimentation. J. P.

I Lost My Memory: The Case as the Patient Saw It. ANON. London, Faber & Faber, 1932. Pp. 305.

This account of gradual emergence from a sudden total amnesia, occurring in the spring of 1931, covering the years from 1911 to 1931, and including an extensive war experience and subsequent scientific career, is written by the patient himself and is a unique document. A letter to the reviewer from a responsible person says that the subject and his physician, who endorses, of course also

anonymously, the account, "are men of first standing in England." The method of treatment consisted largely in causing the patient to call up visual imagery, apparently especially vivid in his case; his dreams were also studied, with a refreshing absence of symbolic interpretation, and free association was used. Various parts of the lost period were gradually recalled in fragments; the final clearing up was sudden, but the events of the first three days after the occurrence of the amnesia were not recovered. The introspective study of the points where resistance appeared is highly interesting. The cause of the amnesia was revealed as certain financial difficulties and apparently the memory of certain domestic difficulties which are not set forth in the narrative, the resistance being still thus far operative! M. F. W.

Secret Ways of the Mind. By W. M. KRANEFELDT. Introduction by C. G. Jung. Translated from the German by R. M. Eaton, with a preface by the translator. New York, Henry Holt & Co., 1932. Pp. xl, 188.

Although purporting to be a "survey of the psychological principles of Freud, Adler, and Jung," this book is essentially a presentation of Jung's system. In a few pages the Adlerian individual psychology is dismissed as a practical system of therapy which has given no answer to the important question 'of the nature of the soul.' Freud's system is discussed at greater length and given more scathing criticism. "The Freudian theory rests on fantasy" and likewise "gives us no theory of the soul." In general the discussion is philosophical rather than psychological. The book is too highly technical for any except the most advanced students and probably does not contain enough new material to interest the specialist.

Harvard University M. H. ELLIOTT

Classic Descriptions of Disease. By RALPH H. MAJOR. Springfield, Ill., Charles C Thomas, 1932. Pp. 630.

In a recent number of this JOURNAL (44, 1932, 590 f.) I presented a review of Fulton's *Selected Readings in the History of Physiology.* The present volume may be considered as doing for the classic accounts of disease what Fulton did for classic physiology. Major's volume is, however, of less professional interest to the psychologist.

Among the early accounts of disease, which may be of interest to psychology, are the following: *Tetanus* as described by Aretaeus the Cappadocian (Second or Third Century A.D.); *Tetany* as described by John Clarke (1761-1815); *Fröhlich's Disease* as described in 1901 by Alfred Fröhlich; *Apoplexy* as described by Johann J. Wepfer (1620-1695); *Erythomelalgia* as described by S. Weir Mitchell (1830-1914); and especially ten selections on *Thyroid Disease,* by writers from Paracelsus to Murray.

Brown University LEONARD CARMICHAEL